

# **RSALOP**

*Radionuclide Soil Action Levels  
Oversight Panel*

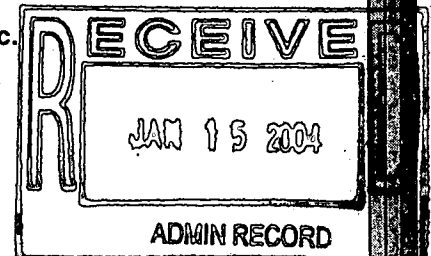
## **Risk Assessment Workshop 2/11/99**

*Compiled by:*



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(303) 456-0884 fax. (303) 456-0858

**1998/1999**



SW-A-004865

1/2601

anna

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From: Kathleen Meyer [kmeyer@verinet.com]  
Sent: Thursday, February 04, 1999 3:51 PM  
To: acorbett@aimsi.com  
Subject: Re: RFSALOP Workshop

Anna,

I assume that Dr. Meinhold will have his slides in a tray but will call his office first thing in the morning to find out.

I will be there by about noon before Dr. Meinhold is scheduled to arrive. Jill Weber and John Till will follow. I will be making copies of his slides for distribution. Should I go ahead and make about 50 copies? Or do you expect more or fewer people to attend the workshop?

Thanks.

Kathleen

At 03:19 PM 2/4/99 -0700, you wrote:

>Kathleen,

>

>I just have a couple of quick questions regarding logistics for the  
>Workshop next week. Does Dr. Meinhold have his slides in a circular  
>carousel for the 35mm projector? Does he need a carousel? Are one of you  
>coming with Dr. Meinhold? Who will be coming? What time are you planning  
>to arrive at the Broomfield Center. Let me know. Anna.

>

anna

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**From:** Carla & Frank Sanda [candfrvl@email.msn.com]  
**Sent:** Monday, February 08, 1999 1:57 PM  
**To:** Anna Corbett  
**Subject:** Logistics

Hi Anna - City of Westminster has easels we can use -- I've asked Mary Harlow for a total of 9 -- 7 for the storyboards, and 2 for flipchart paper. We need to provide 2 pads of flipchart paper and pens. As we get closer to the public meeting, we also need to work with Mary on the sound system and audio visual requirements that RAC representatives may need. I'm going to schedule a teleconference with RAC and the Steering Committee prior to the public meeting to nail down all loose ends.

Now for the Risk Workshop - in my car I have a screen loaned to us by Ken Korkia. I'll drop that, the extension cord, and table tents off to you prior to Thursday.

I'm waiting for some info on Dr. Meinhold to include with the agenda and workshop reminder that will need to be faxed out tomorrow.

I'll e-mail that stuff over as soon as I get it.

Thanks, Carla

# RADIONUCLIDE SOIL ACTION LEVEL OVERSIGHT PANEL RISK WORKSHOP ATTENDEES

February 11, 1999

NAME	ORGANIZATION	PHONE	FAX	E-MAIL
Anna Corbett	AMS1	303 456-0884	303 456-0858	SI.COM
Kathy Salnoor	City of Broomfield	303 438-6363	303 438-6234	userve.com
VEN SPARE	JEFF CO HEALTH DEPT	303 271-5714	303 271-5702	W, CO, 445
Laura Brooks	Kaiser-Hill	303- 966-6130	303- 966-5001	
Tom Pentecost	Colorado Dept of Health Lab and Radiation Services	303 692-3078	303 692-3692	
Pick Roberts	City of Mont. Radiation Serv.	303 966-4869	303 966-3407	
Victor Holm	RFCAB	(303) 989-5086	(303) 980-9076	
Jill Weber	RAC	303 361-4471	605 361-4488	
Kathleen Meyer	RAC	970 229-0828	970 229-0829	
Charles Binkley	NCRP	301 657-7652	301 8768	
Todd Margulies	TM Consulting	303 279-6699	SAME	
Steve Gunderson	CDPHE	303- 692-3367	303 754-5355	
Carol Lyons	City of Anschutz			

February 11, 1999

NAME	ORGANIZATION	PHONE	FAX	E-MAIL
Tom Marshall	Rocky Mountain Region and Science Center	303-444- 6981	303-444- CS25	
JOEL SELBIN	UCRS Chem. Dept	303 444 7341	303 444- 5894	
Carl Spreng	CDPHE	303-692- 3358	303- 759-5355	
Ken Korkia	RIFAB	303 420- 7855	303-420 7579	
LeRoy Moore	RMPJTC	303-444- 6981	303-444- 6523	
HANK STOVALL	CITY OF BROOMFIELD	303-466- 5986	303-438 6296	
Theresa Harland	City of Westminster	303-430- 2400 ext 2174	303 650-1443	
John Corsi	Kaiser-Hill	303 966-6506	303 966-6153	
Russell McEllister	DOE/RFFO	303 966-9692	X 3710	
Nancy M. Daugherty	CDPHE - HMMMD	303 692-3417	303 759-5355	
Willi Nott	RFLA	303/940-6090	303/940-6088	
Brody Wilson	CAD	303 480-7855	7579	

b. org



*Rocky Flats Soil Action Levels Oversight Panel*

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**RISK WORKSHOP**  
**Thursday, February 11, 1999**  
**12:00 - 3:30 p.m.**  
**Bal Swan Conference Room**  
**Broomfield City Center**

**ACTION ITEMS**

**Presentation**

- Kathleen Meyer called to inquire if a slide projector could be made available for Dr. Meinhold. He can use either slides or overheads. Confirm availability of overhead and/or slide projector and advise Kathleen. If he is planning to distribute handout materials, please ask for 50 copies.

**Refreshments**

- Participants were told to provide their own brown bag lunch. Hank Stovall indicated at the January meeting that City of Broomfield would provide refreshments -- beverages and snacks. Please check with Hank to see how this should be arranged. Is this something that we order through a Broomfield supplier, or is this something we work through Diane Eisman?

**Room Set-Up**

- Ask Kathleen what room setup Dr. Meinhold would prefer -- I'm assuming classroom.
- Contact Diane Eisman who will arrange with bldg svcs to arrange room accordingly.
- Work with Diane to ascertain refreshment set-up, replenishment

# **REMINDER**

## **RISK WORKSHOP**

**WHEN:** Thursday, February 11, 1999  
12:00 - 3:30 p.m.

**WHERE:** Broomfield City Building, One Descombes Dr. - Zang's Spur/Bal Swan  
Conference Rooms (lower level)

**KEYNOTE**

**SPEAKER:** *Mr. Charles Meinhold will be conducting the workshop and brings with him a diversified foundation in both the nuclear industry and risk issues, including:*

- *President, National Council of Radiation Protection Measurements*
- *Vice Chairman, International Commission on Radiological Protection*
- *Senior Scientist, Brookhaven National Laboratory*
- *Past President, International Radiation Protection Association*
- *Honorary Professor, The China Institute of Atomic Energy*
- *Honorary Professor, The China Institute of Radiation Protection*

Attendees may bring a brown-bag lunch if desired. Beverages & light snacks will be provided.

### **RSALOP TECHNICAL DISCUSSION**

To provide time for the Risk Workshop, no technical discussion will be conducted prior to the regular Panel meeting.

### **UPCOMING RSALOP MEETINGS**

All future meetings will be held from 4 - 7 p.m. at the Broomfield City Building, One Descombes Dr., Broomfield, CO - Zang's Spur/Bal Swan Conference Rooms, on the following dates:

March 11	April 8	May 13
June 10	July 8	August 12
September 9	October 14	November 11

### **PUBLIC MEETING**

The first public meeting will be held from 6:30 - 9:00 p.m. on Wednesday, March 10, 1999 at the Westminster City Hall - 4800 W. 92nd Avenue - Westminster, CO 80030

Presentation

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# **RADIATION HEALTH RISK ASSESSMENT**

**Charles B. Meinhold**

**President**

**National Council on Radiation Protection and Measurements**

**Vice Chairman**

**International Commission on Radiological Protection**

**Senior Scientist**

**Department of Advanced Technology**

**Brookhaven National Laboratory**

**(301) 657-2652 (phone)**

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- 2 As we begin our discussion of radiation health risk assessment, we need to define our terms. Risk, as used by the Nuclear Regulatory Commission, in the context of probabilistic risk assessment (PRA) uses the "engineering" definition. In this course, we will be using a somewhat more relaxed approach to what we mean by risk, i.e., more closely aligned with the "English" definition. Very often we will use risk to describe the probability of fatal cancer per unit of exposure. In fact, the derivation of our "risk" estimate of  $4 \times 10^{-4}$  fatal cancers/rem is a primary objective of this course. For example, in our epidemiological discussions, we define risk as, "probability that in a given time period a healthy individual becomes ill of the disease of interest."

# DEFINITIONS OF RISK

- Engineering:

*"The potential realization of undesirable consequences from hazards arising from a possible event."*

(McGraw Hill Dictionary of Scientific and Technical Terms)

- English:

*"1. Exposure to the chance of injury or loss. A hazard or dangerous chance."*

(Random House Dictionary of the English Language)

- 12
- 3 The ICRP was formed in 1928 by the International Congress of Radiology. The driving force was the introduction in 1920 of a powerful new Coolidge X-ray tube coupled with the early experience with skin burns among many of the practitioners and their patients. In addition, there were numerous newspaper articles about anemias among X-ray technologists returning from World War I.

At this first meeting, the chairman of the Advisory Committee on X-ray Radium Safety asked each of the representatives to go home and form a national organization so as to insure a consensus approach. The U.S. representative to that meeting was Lauriston S. Taylor from the National Bureau of Standards. He returned home and formed the Advisory Committee on X-ray and Radium Safety. Both the NCRP and the ICRP evolved over the years, and in 1964, the NCRP received a charter from Congress. It is interesting to note that Dr. Taylor presided over the NCRP and its predecessor organizations for 49 years.

The UNSCEAR arose out of international concern over atmospheric nuclear weapons testing. Merrill Eisenbud, then with AEC's Health and Safety Laboratory, went to Japan to check up on the fishermen who had been on the Lucky Dragon fishing vessel. The Lucky Dragon had been in the fallout plume from the Bravo nuclear weapon's test of 1954, a plume which also contaminated the Marshall Islands. While Eisenbud was measuring high levels on the vessel, the U.S. War Department was stating that the skin burns seen in the Marshall Islanders and in the fishermen was due to chemicals in the coral sand. The international outcry resulted in the formation, not only of the UNSCEAR, but the establishment of the BEAR Committee of the National Academy of Sciences, and in the U.K., the establishment of the Medical Research Council's Committee on the Hazards to Man of Nuclear and Allied Radiations. The BEAR Committee has been episodically revived under the acronym BEIR.

**NCRP** National Council on Radiation  
Protection and Measurements

**ICRP** International Commission on  
Radiological Protection

**UNSCEAR** United Nations Scientific Committee on  
the Effects of Atomic Radiation

**NRC:BEIR** National Research Council: Committee  
on the Biological Effects of Ionizing  
Radiations

- 14
- 4 In general, we will be introducing concepts and quantities throughout the text, and these three quantities are the ones we need to begin our discussions. Throughout this text, I will tend to introduce both the traditional quantities, that are those that we have used in the radiation protection jargon since the early 1950s, and also the new international system of units (S.I.) quantities, which have been adopted throughout most of the world about 20 years ago. The international system has adopted the MKS (meter, kilogram, second) system of quantities, rather than the CGS (centimeter, gram, second) system of quantities. In addition to the difference in these traditional and S.I. quantities, we will also introduce some of the new quantities presented in ICRP Publication 60, which modifies the terms which they introduced in Publication 26. For example, dose equivalent has been somewhat changed in the way it has been defined, and ICRP has therefore changed it to equivalent dose. I will also throughout this text indicate the Nuclear Regulatory Commission's system of quantities and units.

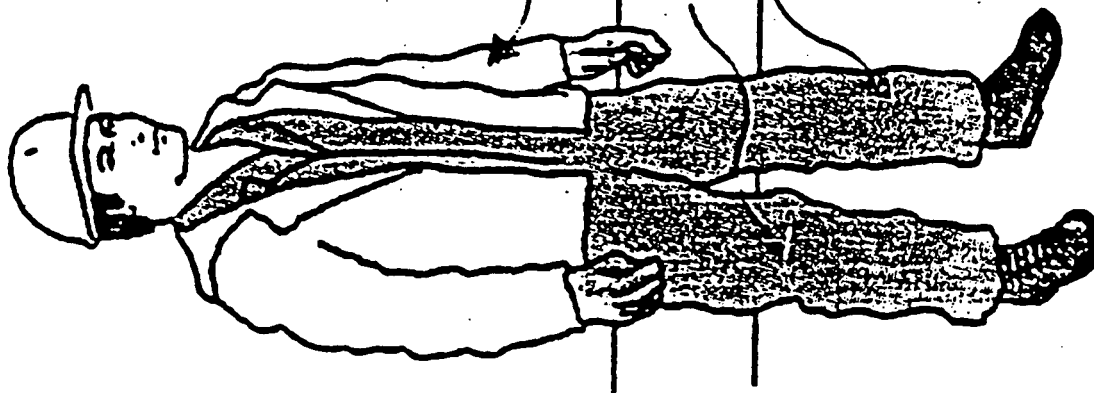
## CONCEPTS AND QUANTITIES

- Activity
- Absorbed Dose
- Dose Equivalent  
(Equivalent dose)

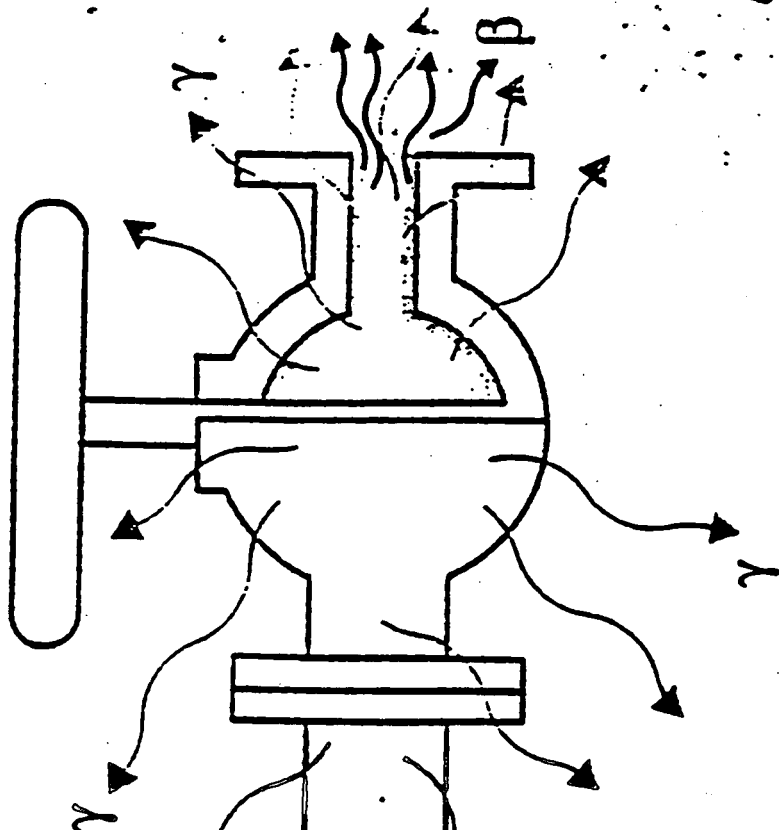
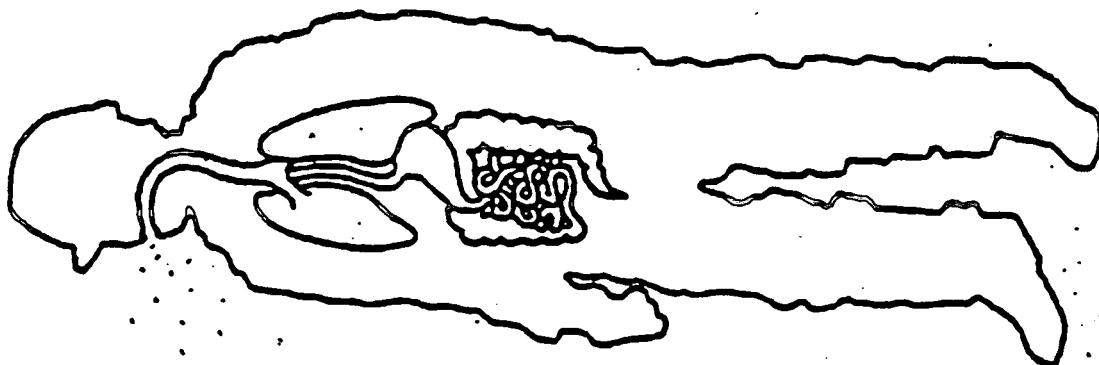
- 16
- 5 One of the confusing issues for the non-expert is the distinction between radiation and contamination. Contamination is composed of particles which contain radioactive materials. These can be inhaled or ingested or rarely enter through the skin. Once in the body they can radiate the individual tissues and organs. For radioactive material which stays put (remains in a sealed source, is attached to or contained by, as in our example, a pipe), it is the emitted radiation which must travel through the container, the air, and eventually reach us. Then we have been exposed to external radiation. For contamination, which leads to internal radiation, we use respirators, gloves, and coveralls. For external radiation, none of these techniques are of any value and we must use time, distance, and shielding to reduce exposures.

Time (reduce the length of time in vicinity of the radian source); Distance (in general, the amount of radiation follows an inverse square law, i.e., at one-half distance, the exposure increases by a factor of 4); Shielding (the radiation is absorbed and scattered by any material between you and the source).

Radiation  
External Hazard



Contamination  
Internal and  
External Hazard



- 12
- 6 Many might think that the activity is the amount of the radioactive material in the source -- but, in fact, it is not. Activity is a measure of the decay of the radioactive material and it is directly related to its half life. As an example, let us suppose that we have an activity of 10,000 disintegrations per second of tritium and 10,000 disintegrations per second of Plutonium-239. Under these conditions, we would have approximately  $5.5 \times 10^{12}$  atoms of tritium and for plutonium a total number of atoms of about  $1 \times 10^{16}$ . We see here that for the same level of activity, which is the same number of disintegrations per second, we have 10,000 more atoms of plutonium than we have of tritium.

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## ACTIVITY

The intensity of a radionuclide source is given in terms of the average number of spontaneous nuclear transformations taking place per unit time.

- 20
- 7 The first entry in the overhead is the definition of a curie, which is  $3.7 \times 10^{10}$  disintegrations per second. This was based on the activity associated with 1 gram of radium. Of course, it is a somewhat awkward quantity to use, whereas in the S.I. system, 1 Becquerel (Bq) is 1 dis/sec. On the other hand, small quantities of radioactivity appear to be very large in terms of the number of Becquerels associated with them. You notice, for instance, that 27 millicuries is 1 GBq ( $10^9$  dis/sec). This chart also allows you to review the factor of three between the prefixes. For instance, a millicurie is  $10^{-3}$ , or a thousandth of a curie, and a thousandth of a millicurie is a microcurie, a microcurie being  $10^{-6}$ , and a nanocurie is  $10^{-9}$ , and so on.

In the S.I. system, as given in the chart, goes the other way. The Bq, the next  $10^3$  more is a kilobecquerel (KBq),  $10^3$  more than that is a megabecquerel (MBq), and  $10^3$  more than that is a gigabecquerel (GBq).

# THE UNITS FOR ACTIVITY

Traditional	Activity	S.I.*
1 curie	$3.7 \times 10^{10}$ dis/sec	$3.7 \times 10^{10}$ Bq
1 millicurie	$3.7 \times 10^7$ dis/sec	$3.7 \times 10^7$ Bq
1 microcurie	$3.7 \times 10^4$ dis/sec	$3.7 \times 10^4$ Bq
1 nanocurie	$3.7 \times 10^1$ dis/sec	$3.7 \times 10^1$ Bq
27 picocuries	1 dis/sec	1 Bq

S.I.*	Activity	Traditional
1 Bq	1 dis/sec	27 picocuries
1 KBq	1,000 dis/sec	27 nanocuries
1 MBq	$10^6$ dis/sec	27 microcuries
1 GBq	$10^9$ dis/sec	27 millicuries
37 GBq	$3.7 \times 10^{10}$ dis/sec	1 curie

\*Abbreviation for International System of Units

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- 8 In this section we will be discussing dose in some detail and the way in which we modify the simple quantity dose, i.e., the absorbed dose, and how we modify the absorbed dose to become the equivalent dose, the effective dose, and even later, the committed dose.

**Committed**

**Effective**

**DOSE**

**Absorbed  
Equivalent**

- 24
- 11 In an attempt to derive a quantity which correlated with biological effects, the ICRU, ICRP, and NCRP adopted the quantity absorbed dose. The classical definition of absorbed dose,  $d$ , is the quotient of  $d\epsilon/dM$ , where  $d\epsilon$  is the mean energy imparted by ionizing radiation to matter of mass,  $dM$ . That is,

$$d = \frac{d\epsilon}{dM}$$

For radiation protection purposes, however, we can have a looser definition. For example, the ICRP uses the quantity  $D_T$  which is the total amount of energy deposited in a given organ or tissue divided by the mass of that organ or tissue.

In the example, we see that very large, absorbed doses are needed to "heat" tissue while that amount of energy can cause serious biological effects.

# ABSORBED DOSE

Absorbed dose is energy deposited per unit mass in matter

Traditional quantity is the rad:

$$1 \text{ rad} = \frac{100 \text{ ergs}^*}{\text{gm}}$$

S.I. quantity is the Gray (Gy):

$$1 \text{ Gy} = \frac{1 \text{ Joule}}{\text{Kg}}$$

$$1 \text{ Gy} = 100 \text{ rad}$$

\* $4.2 \times 10^7$  ergs will raise 1 gm of water one degree Celsius or  $4.2 \times 10^5$  rads will raise 1 gm of water 1 degree Celsius.

- 12 This example, which demonstrates how one goes from ionization in tissue to absorbed dose is somewhat unrealistic in that one wouldn't be very concerned about a situation where there was simply one disintegration in a gram of tissue. However, if you think about this not in a gram of tissue but in a single cell where there might be  $10^{-12}$  grams of tissue, then the dose to that tiny mass of tissue, now  $10^{-12}$  grams, would be 5.4 ergs/gram or  $5.4 \times 10^{-2}$  rads.

## EXAMPLE

Ionization in tissue deposits  $\sim 34 \text{ eV} = 5.4 \times 10^{-12} \text{ ergs}$ .

If our ion pair exists in 1 gm soft tissue, the absorbed dose is

$$\frac{5.4 \times 10^{-12}}{\text{gm}} \text{ ergs}$$

$$\text{Since } 1 \text{ rad} = 100 \frac{\text{ergs}}{\text{gm}}$$

We have  $5.4 \times 10^{-14} \text{ rads}$   
or  $5.4 \times 10^{-16} \text{ Joules}$

- 28
- 13 As noted earlier, there are differences in the way in which ionization takes place in tissues as the function of the particle and the energy of the particles and the type of particles. To account for these differences which do, in fact, result in differing biological effects, a new quantity was developed. This new quantity is the dose equivalent. In ICRP 60 it became the equivalent dose and  $H$  was replaced by  $H_T$  to indicate that we have averaged the absorbed dose over the mass of the entire tissue or organ of interest. The modifying factor used to reflect this enhanced biological effectiveness is called the quality factor. 10 CFR Part 20 also uses  $H_T$  to indicate that it is the product of absorbed dose in tissue and the quality factor. The units of dose equivalent are the rem and the Sv.

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## DOSE EQUIVALENT (EQUIVALENT DOSE)

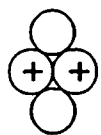
The absorbed dose is adjusted to reflect the greater biological effect associated with particles producing more densely ionizing tracks.

The Dose Equivalent (H) =  
The absorbed dose (D) x a weighting factor ( $\bar{Q}$ )


$$H = D \times (\bar{Q})$$

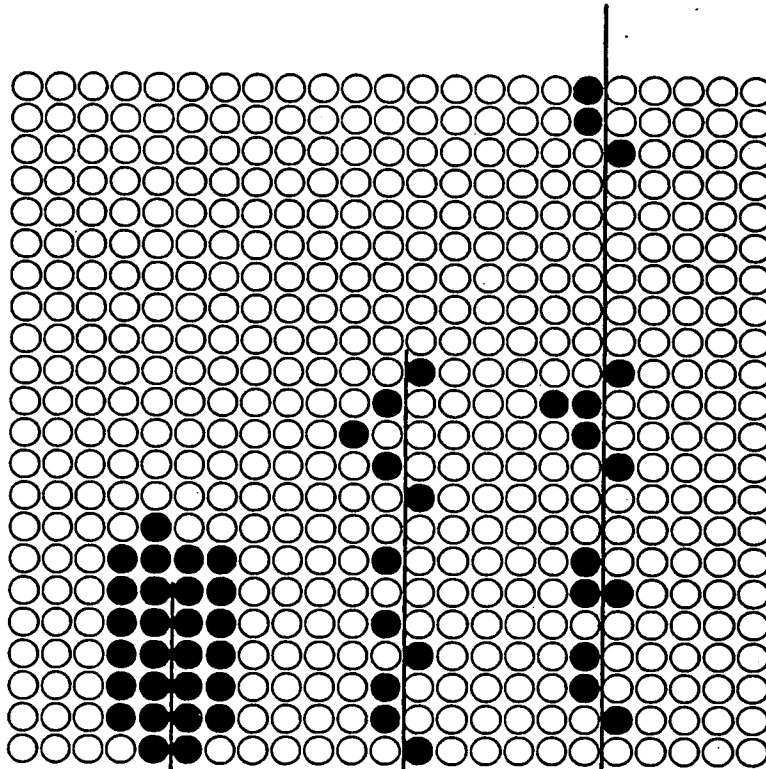
- 30
- 14 This simple diagram may help to visualize the deposition of energy in tissue. Note that the alpha particle, which is composed of two protons and two neutrons, moves through tissue in a way that leaves a large clump of ionization very close to the track. This, of course, is because the particle is large, moves rather slowly, and has two positive charges. Note that the beta particle has more diffuse ionization as it moves through tissue, and the gamma rays, which eventually result in production of secondary electrons, can be simply represented by a series of interactions similar to a number of beta particles which arise along the path.

# RADIATION PATHS IN TISSUE

Alpha particle:   
easily stopped  
least penetrating

Beta particle:  $\ominus$   
very much smaller  
more penetrating

Gamma ray and X-ray:   
pure energy with no mass  
most penetrating



○ neutral atom or molecule  
● ion

- 32
- 15 Note particularly in this chart that the electrons, which indicate that there are anywhere from 5 to 30 ion pairs per micrometer ( $10^{-6}$  meters) in tissue, are the same as it would be for X-rays or gamma rays. Again, the only difference is that an X-ray or gamma ray can cause such ionizations to occur throughout a very long length of path. For instance, as an X-ray or gamma ray passes through the body, any released electrons would produce one track. This table also suggests that neutrons, particularly those in the higher ranges where proton recoils are more likely in around 1 MeV, have extremely high ionization densities associated with them -- up to 3,000 ion pairs per micrometer. Of course, the alpha particles put them all to shame with densities in the range 3,000 to 7,000.

## IONIZATION DENSITY

Radiation	Ion Pairs/ $\mu\text{m}^*$ in Tissue
electrons	5-30
$\alpha$ particles	3,000-7,000
neutrons (proton recoils)	100-3,000
X, $\gamma$ rays	Many tracks 5-30 widely distributed in tissue

\*1  $\mu\text{m}$  =  $10^{-6}$  meters

- 20 The information in this table is particularly instructive in that it demonstrates our lack of precise knowledge about the hazard of neutron irradiation. This table is concerned with the relationship between gamma rays and fission neutrons. Part of the difficulty in doing any RBE study at very low doses is that the incidence of disease or other biological endpoint related to the gamma ray exposure at very low doses is hard to discern, even in cell cultures. Be that as it may, it is the linear slope of the gamma dose effect curve compared with the linear portion of the neutron dose effect curve, which produce the values given in this table. You notice that these ranges vary much within each endpoint. For instance, for chromosome aberrations in human lymphocytes in culture, the values range from 34-53. Notice in the next endpoint, oncogenic transformations, the RBE values are as wide as 3-80, the next 5-70, 2-100, 10-46. Now look at the range between each endpoint, 53-80, 53-100, 46-59. Clearly, not only is there a wide range of values with each endpoint, but even among endpoints. The data do suggest, however, that neutrons are much more efficient at producing damage in these endpoints than gamma rays or X rays. How then, should we use this information in setting our recommendations?

# **SUMMARY OF ESTIMATED RBE<sub>M</sub> VALUES FOR FISSION NEUTRON VERSUS GAMMA RAYS**

(Adapted from Table 4.1 of NCRP Report 116)

End point	Range of values
Chromosome aberrations, human lymphocytes in culture	34 - 53
Oncogenic transformation	3 - 80
Specific locus mutations in mice	5 - 70
Mutation end points in plant systems	2 - 100
Life shortening in mice	10 - 46
Tumor induction in mice	16 - 59

- 23 This overhead demonstrates one of the ICRP's basic principles which is that we imply far too much precision in our recommendation than is warranted by the biological information. ICRP has suggested a weighting factor which applies to the radiation field which we could apply broadly, that is, one over all energies for electrons, muons, and photons. For neutrons  $<10$  keV/ $\mu\text{m}$  it is five. For neutrons between 10 keV and 100 keV, it's 10 and from 100 keV to 2 MeV, it's 20. From 2 MeV to 20 MeV it drops down to 10. Then it drops further down to 5 for protons, and for alpha particles and their fragments, it's 20. Part of the reason that it is 20, rather than 30 as might have been suggested by the Q-LET relationship, is that once the neutrons have been transported through the body their effective energy changes, and since we apply the weighting factor to the field, we can take this into account.

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## RADIATION WEIGHTING FACTORS

Type and energy range	Radiation weighting factor, $W_R$
Photons, all energies	1
Electrons and muons, all energies	1
Neutrons, energy <10 keV	5
10 keV to 100 keV	10
>100 keV to 2 MeV	20
>2 MeV to 20 MeV	10
>20 MeV	5
Protons, other than recoil protons, energy > 2 MeV	5
Alpha particles, fission fragments, heavy nuclei	20

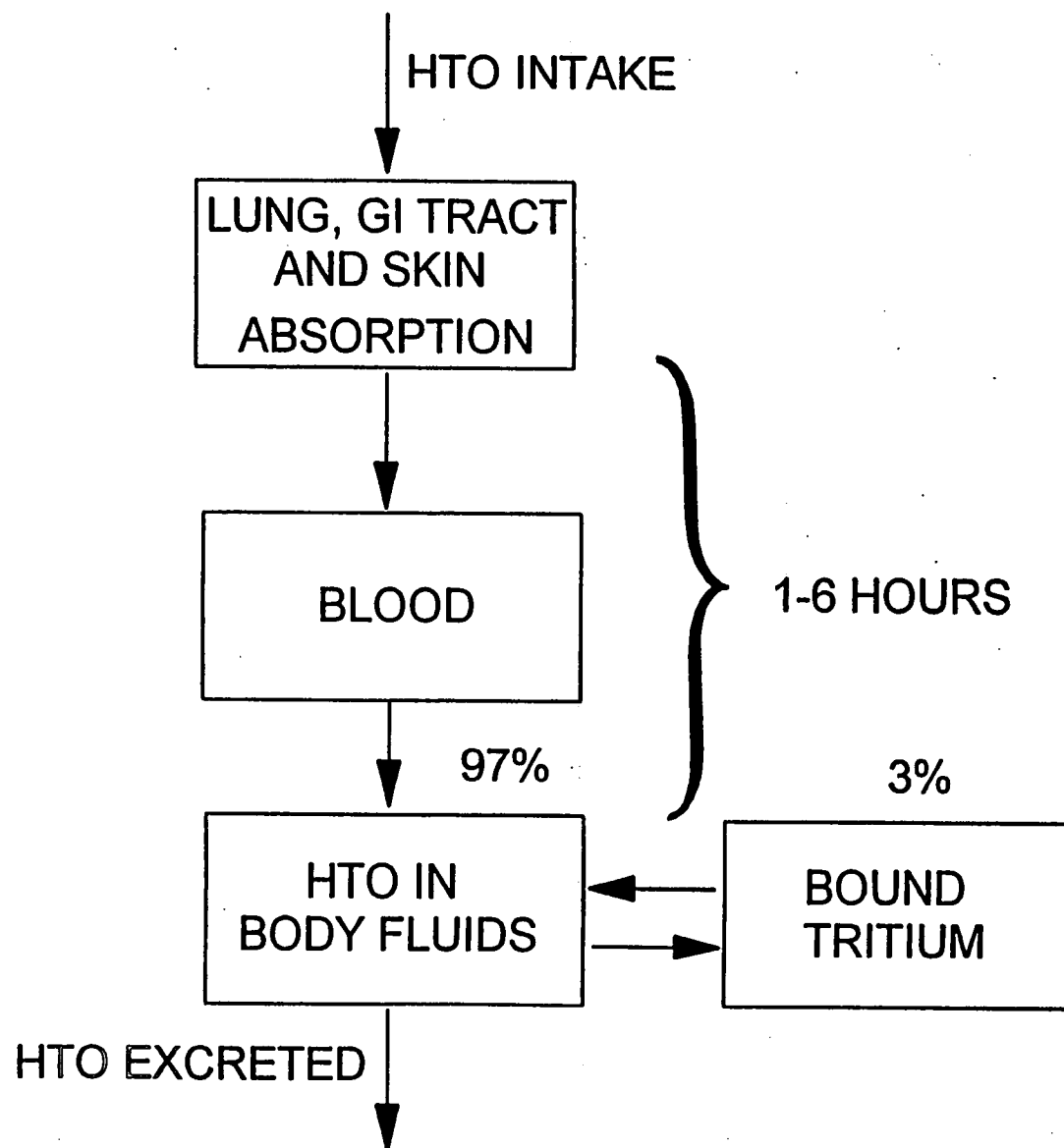
- 24 Up to this point we have been dealing primarily with external radiation, although most of the principles apply when the body is irradiated by materials which are deposited inside the body. However, there are some other considerations which make internal exposure a matter of specific discussion.

## INTERNAL EXPOSURE

- Concepts and quantities
- Simple metabolic model
- Physical and biological half-life
- Dose calculation for internally deposited radionuclides
- Complex metabolic model
- Effective dose
- Tissue weighting factors
- Committed effective dose

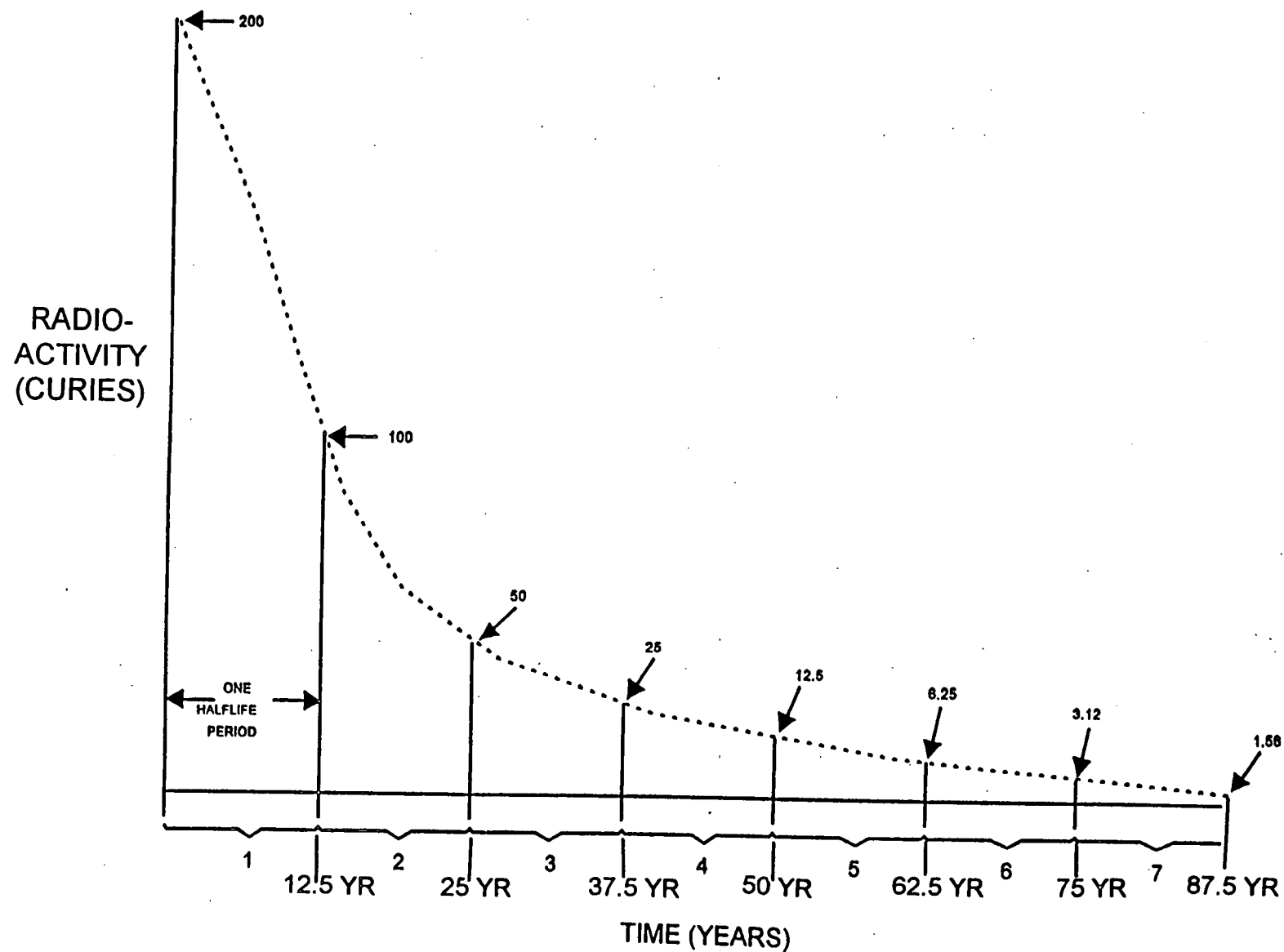
- 25 Metabolism of tritiated water in the body is one of the simplest to model. Tritiated water enters the body through inhalation or through ingestion via the lung or the gastrointestinal tract. Water also passes through the skin as a result of absorption. In about six hours, this tritiated water has reached the blood and all the body fluids, i.e., the tritiated water exists in all the tissues of the body. Notice that about 3% is held up in the organically bound materials. That is not surprising, since a large fraction of the tissue of the body is made up of organic compounds and they are rich in hydrogen. Notice the interface between the tritiated water in the body fluids and that in the bound fraction, i.e., some of the free water becomes bound, some of the bound tritium breaks down and becomes free water and is then excreted as tritiated water. This is a very simple metabolic system because eventually the material travels to every tissue in the body and eventually leaves the body. This is because it acts just exactly like ordinary water.

# TRITIATED WATER (HTO) METABOLISM



- 26 The half-life of tritium is about 12.5 years. This means that in 12.5 years, half of it is gone. About 7.5 half-lives later, about 90 years, our 200 Curies or 200 Bequerels would be only 1.56. If one were thinking of physical half-life alone, one might think that tritium could pose a rather significant radiological hazard in the body. However, let's move onto the next overhead.

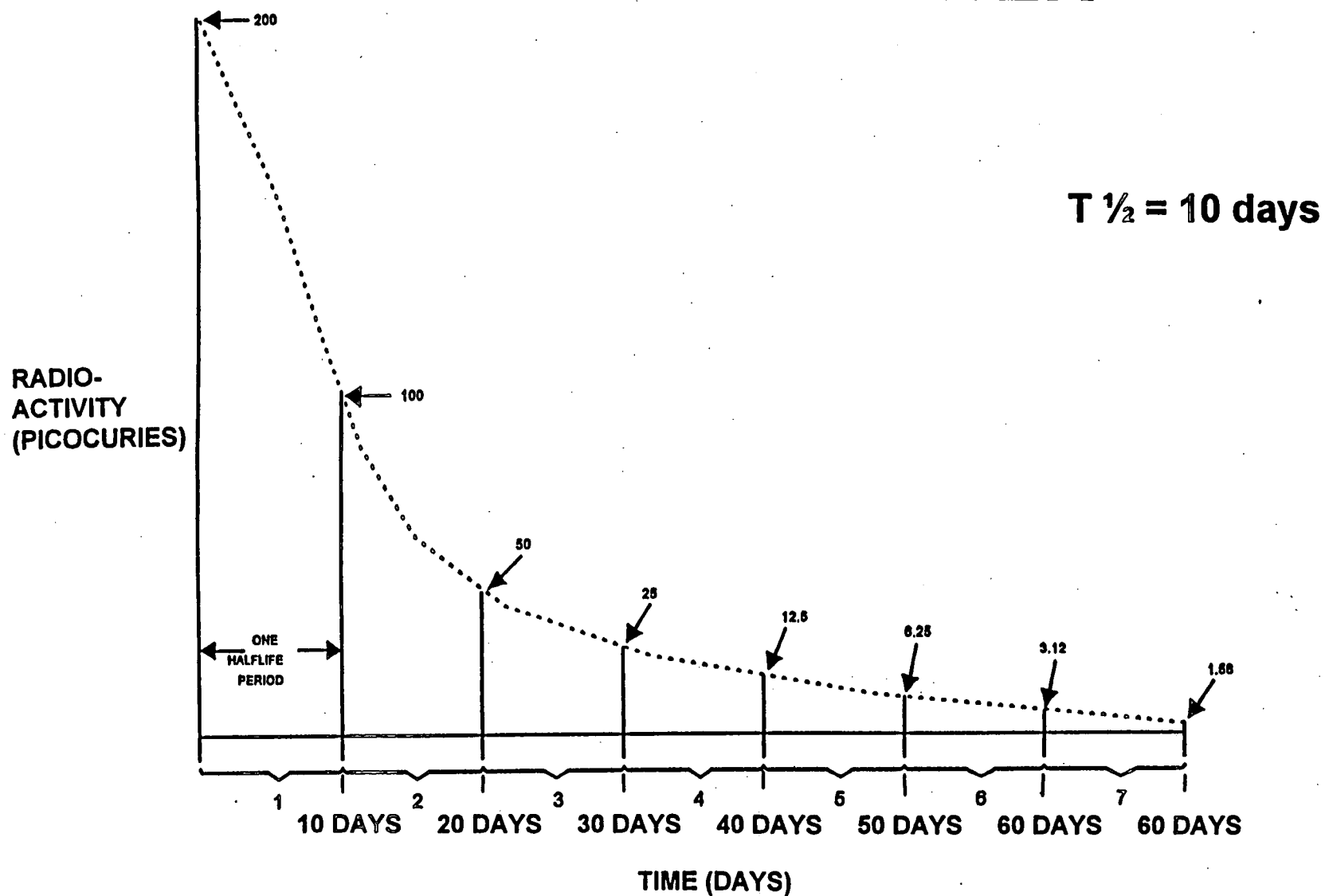
# PHYSICAL HALF-LIFE OF TRITIUM



44  
27

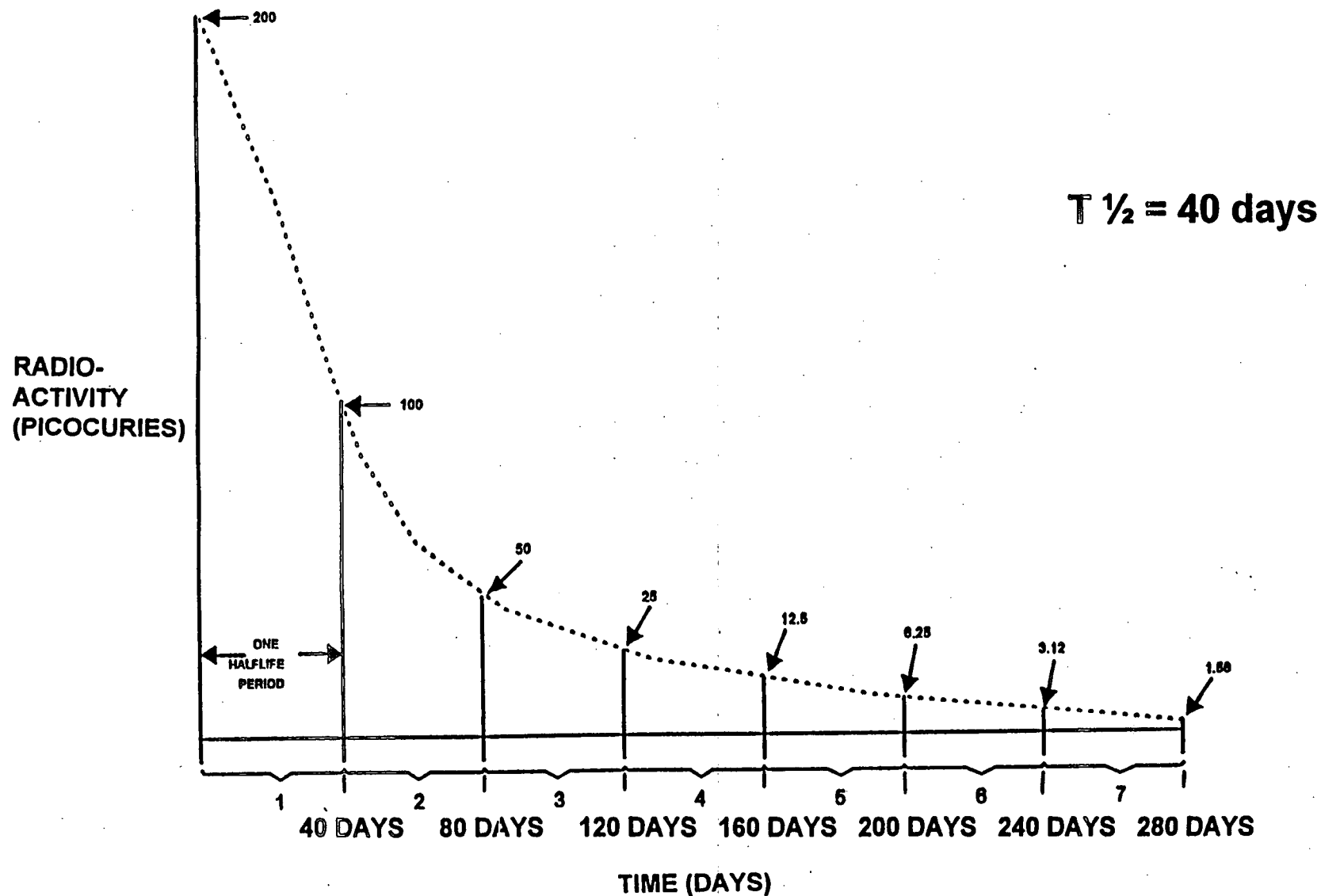
Here, we see the biological half-life of tritiated water. This means that if I have a cc of tritiated water in my body, 10 days later there will be half a cc, etc. Notice that the shape of the curve is very similar to the physical half-life shape, but the values are very different. Instead of a physical half-life of 12.5 years, we now have half a half-life for tritiated water of only 10 days, that is, once you take it in, it leaves the body quite rapidly. That is the reason that the hazard is no where near as great as might be implied by the physical half-life of 12.5 years.

# BIOLOGICAL HALF-LIFE OF TRITIATED WATER



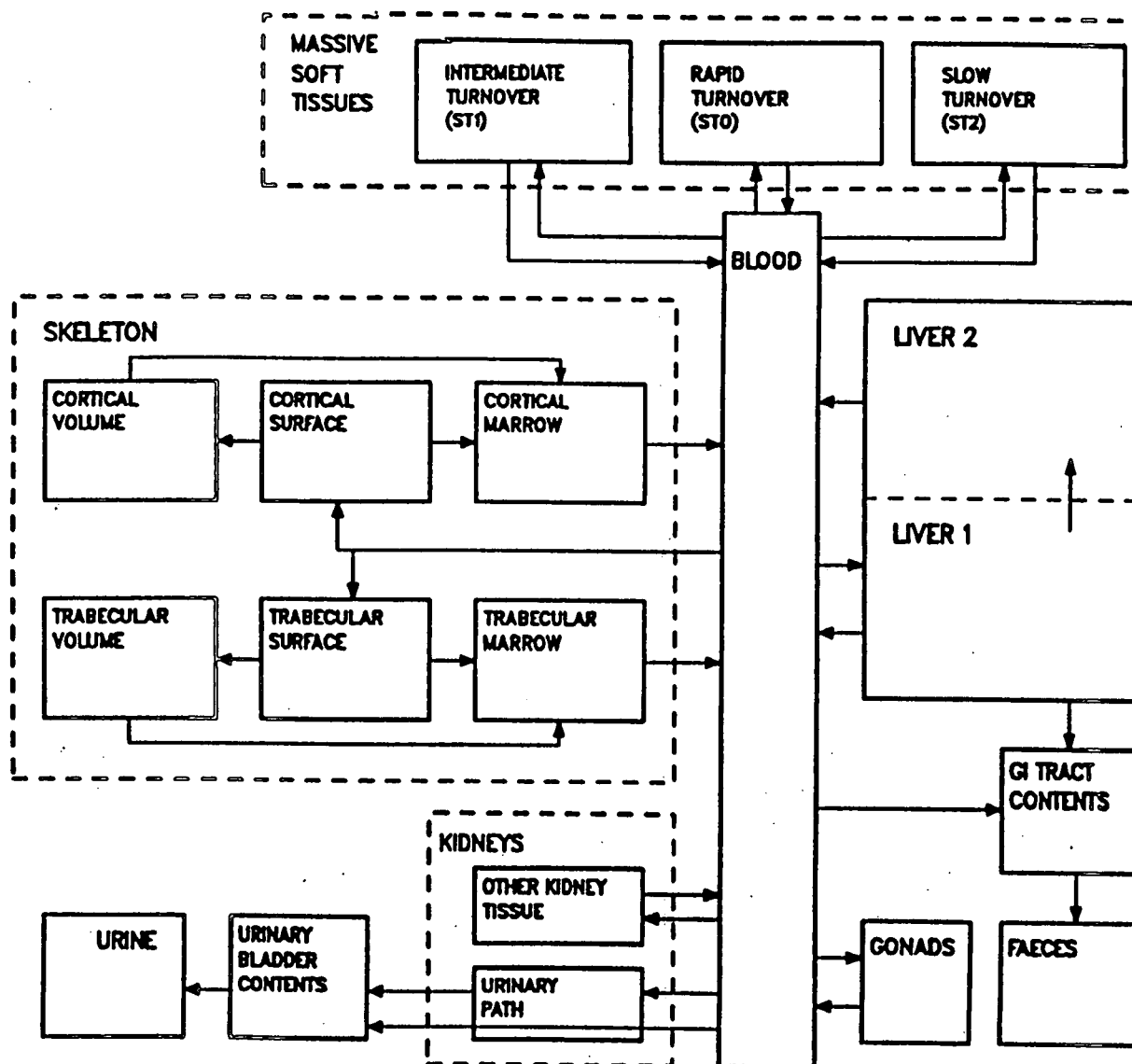
- 28 This is a somewhat different plot, which is biological half-life of the organically bound tritium fraction. Here, the half-life is 40 days which means that over a 40-day period, you would lose half of the bound fraction. Over 80 days, you would be down to a quarter, etc., and follow the same pattern as our other two plots. Recall that this half-time applies to the 3% of the ingested or inhaled quantity which is organically bound.

# BIOLOGICAL HALF-LIFE OF ORGANICALLY BOUND TRITIUM



- SP
- 31 This overhead diagram is a model which is very complex. This model was developed for plutonium, americium, and neptunium. Clearly, we can't do our simple averaging over the body. We can't take a simple approach to how we make the calculation. We've got to know how fast the material moves from the blood into the liver and out of the liver back into the blood. How fast does it go to cortical bone or cortical surfaces? How fast does it get into the urine or feces? All of this is a very complex system and takes a complex computer code to work out the resulting doses. More important, we'll see that all of the different tissues of the body -- the liver, bone surface, marrow, gonads, GI tract, kidney, and bladder -- are going to be exposed albeit to very difficult levels from these materials. We have to have a way in which we can account for the different dose to each of these individual organs and yet still be faithful to our individual dose limits.

# DIAGRAM OF THE BIOKINETIC MODEL FOR PLUTONIUM, AMERICIUM, AND NEPTUNIUM



- 32 The approach to take care of this multi-organ exposure problem where there may be some organs with very high doses and some without is to use what is called the effective dose equivalent. This was introduced in ICRP Publication 26. Our overhead uses,  $E$ , and the NRC Part 20 uses  $H_E$ , for this quantity. The use of effective dose equivalent is an attempt to take the dose equivalent in each organ, weighted by a factor which reflects the fraction of risk to the whole body from that organ, ( $W_T H_E$ ), and then to sum over all the organs to get the total ( $E$ ).

## EFFECTIVE DOSE (EQUIVALENT)

$$E = \sum_T W_T H_T$$

Where

$W_T$  = a tissue weighting factor for tissue,  
T and  $H_T$  = the dose equivalent in tissue, T

- 33 The idea is to derive a quantity which would make any internal exposure or exposure of organs throughout the body equivalent to a whole-body dose. The way this is done, as mentioned in the previous overhead, is by the use of tissue weighting factors. You will notice that the weighting factors used by the NRC in its 10 CFR Part 20 (1995) are ICRP 1977 values. Adding the tissue weighting factors for all the listed organs must, of course, equal one because that is how they are derived. The total risk is apportioned to each tissue. You can see that in 1977, 25% of the risk was assumed to be due to effects on the gonads, the ovaries, and heredity; 12% was leukemia; 12% was lung cancer; 15% was breast cancer, and on through the whole list. You notice that in 1990, a much greater number of organs are listed. This comes primarily from the data in Japan which more and more reflect the effects of the exposure at later times in life to those that were irradiated as children. Again, however, they have to add up to one. Some have been concerned that the  $W_T$  for the breast went from 0.15 in 1977 to 0.05 in 1990. This does not reflect a change in our risk estimates for breast cancer, but rather the influence of adding additional tissues

53

33

## TISSUE WEIGHTING FACTORS

Tissue or Organ	ICRP Tissue weighting factor ( $W_T$ )	
	1977*	1990
Gonads	.25	0.20
Bone marrow (red)	.12	0.12
Colon		0.12
Lung	.12	0.12
Stomach		0.12
Bladder		0.05
Breast	.15	0.05
Liver		0.05
Oesphagus		0.05
Thyroid	.03	0.05
Skin		0.01
Bone surface	.03	0.01
Remainder	.30	0.05

\*Adopted in NRC 10 CFR Part 20 (1995).

- 54
- 34 Thinking back to both our tritium and plutonium models, you recall that we only had a 10- or a 40-day half-life for the biological half-time for the tritium. For plutonium, the half-life can be on the order of 30 or 40 years, depending upon the chemical form. We were bedeviled for many years at how to take this into account. The way in which this finally was resolved is the use of the committed effective dose. The principle behind the committed effective dose is that the person who is controlling the workplace has a responsibility for all of the risk his employees incur during the year. This means that if an employee ingests a large quantity of plutonium, calculations should be made of the effective dose that the individual will receive over the next 50 years. In that way, the responsibilities for that exposure will clearly rest on the person who permitted it to happen. The next year, the contribution from that plutonium is no longer counted, since it has already been accounted for in the previous year. There have been those who argue that because it is the committed effective dose, part of the dose will never be received and some of what is received may never express itself as a cancer. It is important to recognize, however, that this is an average over all ages, so that some of these people will be 18 at the time they ingest the plutonium and are likely to be exposed to other sources of radiation throughout their working life. Notice that for the workers it is over the next 50 years, but for the population it is up to age 70.

The effective dose to be received over the next 50 years is assigned to the year of intake

### Committed Effective Dose

$$E(\tau) = \sum_T w_T \cdot H_T(\tau)$$

Where  $\tau = 50$  years for workers and up to age 70 for members of the public

- 35 Again, thinking back to our complex model of plutonium, americium, and neptunium, we recognize that there has to be the use of chemical analogies. A draft NCRP commentary on uncertainties has pointed out that there is a hierarchy in the way in which we should characterize the information we have on internally deposited radionuclides, particularly in regard to metabolism. Probably the most important single issue is knowledge about the transfer from the gut to the blood, called the  $f_1$  factor. When we have direct information from humans, we feel very secure in the knowledge we have. For example, with Strontium-90, we have good information because of the worldwide contamination from atmospheric weapons tests. However, our direct information for most compounds is very limited, so we must use chemical analogies. In our model, we went from some knowledge about plutonium to neptunium. Such analogs are still quite good because metabolism is largely a chemical process. Knowledge of still lower value is obtained from information on animals. Radioactive materials are fed to animals and the metabolism is determined. Again, if it's just on animals, the hierarchy of that information clearly is lower than that from humans and we have to be more concerned over whether or not we have usable data. An even lower value is obtained when we use a chemical analogy on animals as our way of getting the needed information. Therefore, what both ICRP and NCRP are looking at is a way of developing a subjective evaluation of reliability for these four approaches.

## CATEGORIZING INFORMATION USED IN INTERNAL DOSE ASSESSMENT

Value



- Direct information on humans
- Chemical analogy for humans
- Direct information on animals
- Chemical analogy for animals

∴ Subjective evaluation of reliability

- 36 That is precisely what is given here. For instance, for the adult male who has received tritium through ingestion, the uncertainty in the dose is assigned a factor of three. This means that the value obtained from this model could overestimate the dose by a factor of three and could underestimate the dose by a factor of three. You will notice for infants that it could be a factor of 5. The reason is that in Publication 30, the organic fraction was underestimated, but it has been corrected in Publication 60. For Iron 55, we see an uncertainty of a factor of 5 in each direction, with a factor of 10 for teenage females, and here, everything is uncertain: the dosimetry, the  $f_1$  values, and the biokinetics. I give you examples here, not of everyday compounds, but more to give you the idea of the range of the uncertainty which we have in some of these materials. So you see that we have even greater than a factor of 10 for this last compound, because it is based primarily on rat data, and we know very little about what that really means for man. So we think it could overestimate the dose by a factor of 10 or it could underestimate the dose by a factor of 10.

# **SUBJECTIVE ESTIMATION OF UNCERTAINTY FOR SELECTED RADIONUCLIDES OF THE EFFECTIVE DOSE COEFFICIENT VALUES RECOMMENDED IN ICRP PUBLICATION 30**

Radio-nuclide	Mode of Intake	Adult Male	Special Group	Comments
H-3	Ingestion	Factor of 3	Factor of 5 (infants)	The dose from the organic fraction (HTO) may have been underestimated in ICRP 30, especially for infants
Fe-55	Ingestion and inhalation	Factor of 5	Factor of 10 (teenage females)	Uncertain dosimetry, f1 values, and biokinetics
Pd-103	Ingestion and inhalation	Factor of >10	>10	Based on rat data (f1 value and biokinetics)

- 37 You will note that two entries are given for Dose Equivalent, i.e., Equivalent Dose. The Dose Equivalent is the quantity given in ICRP 26 (1977) and 10 CFR 20 today. The ICRP/NCRP have adopted a similar quantity called the Equivalent Dose. The Effective Dose (ICRP 60) is a somewhat altered version of the Effective Dose Equivalent (NRC) and the Committed Effective Dose (ICRP 60) is an altered version of the Committed Effective Dose Equivalent (NRC).

## REVIEW OF QUANTITIES AND UNITS

	Traditional	S.I.
Activity	Ci $3.7 \times 10^{10}$ dis/sec	Bq 1 dis/sec
Absorbed dose	rad $100 \text{ erg gm}^{-1}$	Gray $1 \text{ Joule gm}^{-1}$
Dose equivalent Equivalent dose	rem $\text{rad} \times \text{QF}$	Sv $\text{Gy} \times W_R$
Effective dose	rem $\sum W_T H_T$	Sv $\sum W_T H_T$
Committed Effective Dose	rem	Sv

- 62
- 38 Now we begin our discussion on a broad range of issues, first, dealing with biological effects. Even that discussion begins with terminology on what we mean by different kinds of exposures and by different kinds of effects. We'll talk briefly about Acute Radiation Syndrome, a subject I am not sure belongs in this topic, but I suppose you can't have a discussion on radiation effects without including some of it. Then, we will take a look at the history of an effects-based dose limitation system from the point of view of how it was done from the 1920s to ICRP Publication 26, which is the basis for much of what is in 10 CFR Part 20 (Revised). We will then begin to look at how some of our estimates of risk have changed since that time.

## BIOLOGICAL EFFECTS

Exposure to effects

Acute Radiation Syndrome

History of effects-based dose limitation

- Threshold
- Linearity
- Genetics

Emergence of solid cancer in the Japanese survivors

- 39 I don't suppose that anything has bothered me more as I have gone through NRC licensee training courses in radiation effects in order to gain plant admittance than a confusion about these terms. People talk about acute effects and chronic effects, but, in fact, I'll try to show you that with one single exception that is the exposure that is acute or chronic and the effects that are either stochastic or nonstochastic/deterministic. I think deterministic is somewhat more descriptive than nonstochastic. The point of this figure is that you can have an acute exposure which can lead to both stochastic and deterministic effects depending upon the level of the exposure. You can have chronic exposures which can go both ways as well, can lead either to stochastic or deterministic effects, depending primarily upon the level of exposure.

## Exposure

## Effects

**Acute**



**Chronic**



**Stochastic**

Probability of Cancer  
is proportional to dose

**Deterministic (nonstochastic)**

Severity increases with  
dose above a threshold

- 40 In order to explain this idea a little more, let's think about two kinds of acute exposure. Let's suppose you are exposed over an hour to 10 rads. There is no probability of a deterministic or nonstochastic effect at this level. All that will result are stochastic effects limited to increasing the probability of cancer or severe genetic effects about one-half a percent.

Now let's take a look at acute exposure, that is, 200 rads. Now there are some deterministic effects which you are likely to see: depressed white blood cells, red blood cell precursors, some problems with infections, temporary sterility, and you will have to take care to have the individual carefully medically supervised. But there is also, however, a substantial increase in the stochastic effects and the probability of cancer and genetic effects go to about 20%. This is on top of the existing probability of cancer of 17%.

19

40

## ACUTE EXPOSURES

- 10 rads** - Limited to increasing the probability of cancer or genetic effects (.5%)
- 200 rads** - Deterministic effects likely -- depressed white blood cells and red blood cells precursors. A substantial increase in the probability of cancer and genetic effects (20%)

- 62
- 41 Let's look at two examples in which the exposure is delivered in a chronic manner, that is, 100 mrem a week for 10 years. Here there would be some increase in the probability of cancer and genetic effects -- again, about 0.5%. Now let's look at 1 rem every day for 10 years. Two things are different. There's going to be a substantial increase in the probability of cancer from our case above from about 0.5% to  $\approx 100\%$ . In addition, there's also a strong likelihood of later complications of anemia and sterility from such exposures. Those, of course, would be deterministic.

## CHRONIC EXPOSURES

- 100 mrem/wk for 10 years** - Some increase in the probability of cancer and genetic effects (stochastic). No deterministic effects.
- 1 rem/day for 10 years** - Substantial increase in the probability of cancer (stochastic). Increased likelihood of anemia and sterility (deterministic).

- 20
- 47 This is an interesting continuation of our deterministic effects because it demonstrates that in 1925, the first limitation we had on radiation exposure was based on the deterministic effect, erythema. Erythema is a skin burn. It is the breakdown of the surface tissues and the blood supply to them and it can ulcerate and cause severe damage to the skin. This was something that was recognized from within 30 days of discovery of the X-ray. Erythema burns of the skin had been noticed both in the patients and in the practitioners. By 1925, however, things had gotten much worse. Around 1920, an engineer named Coolidge had invented a tube which could carry much greater currents at much higher energies. The practitioners themselves became very nervous about what might happen. In fact, they petitioned their associations to help them with this problem. There were many people looking for ways in which these exposures could be controlled. I should point out that it was during this period following the first World Wars that there had been reports of a lot of people coming home with anemia that had been exposed to ionizing radiation in the battlefield, particularly the medical technologists. This was all showing up in the local newspapers. In 1925, an erythema dose limit was suggested.

11

47

**1925**

**“Erythema” Dose Limit**

- 12
- 48 This is the erythema dose limit. Mutschler, in New York, suggested that they shouldn't get more than  $1/100$  of an erythema dose in a 30-day period. Most of these practitioners were fully aware of the situation with regard to erythema. They knew that at certain distances from their X-ray tubes, if you let the patient be exposed for more than, let's say 10 minutes, there would be a serious burn. It was in this way that this recommendation was used. In fact, you had to limit your time in these areas very severely so that you were well below the exposure to X-rays that would result in an erythema. You had to be at  $1/100$  of that (6 seconds, in our example), in any 30-day period. Sievert working in Sweden came up with  $1/10$  of an erythema dose in one year. You will notice that it is the same recommendation within a factor of 20%. It was really amazing that they should both come up with this erythema dose primarily based on the fact that this would keep their staff from getting erythema, but that they could still do their job in well-run clinics where no one had to be exposed to more than  $1/100$  of an erythema dose in 30 days.

73

48

## ERYTHEMA DOSE LIMIT

- Mutschler       $1/100$  Erythema dose in 30 days
- Sievert         $1/10$  Erythema dose in 1 year

- 49 I took one phrase from Mutschuler's paper which demonstrates one of the important concepts of protection at that time. Of course, that is one of toleration. It was "...the dose which an operator can, for prolonged period of time, tolerate without suffering injury."

## MUTSCHULER

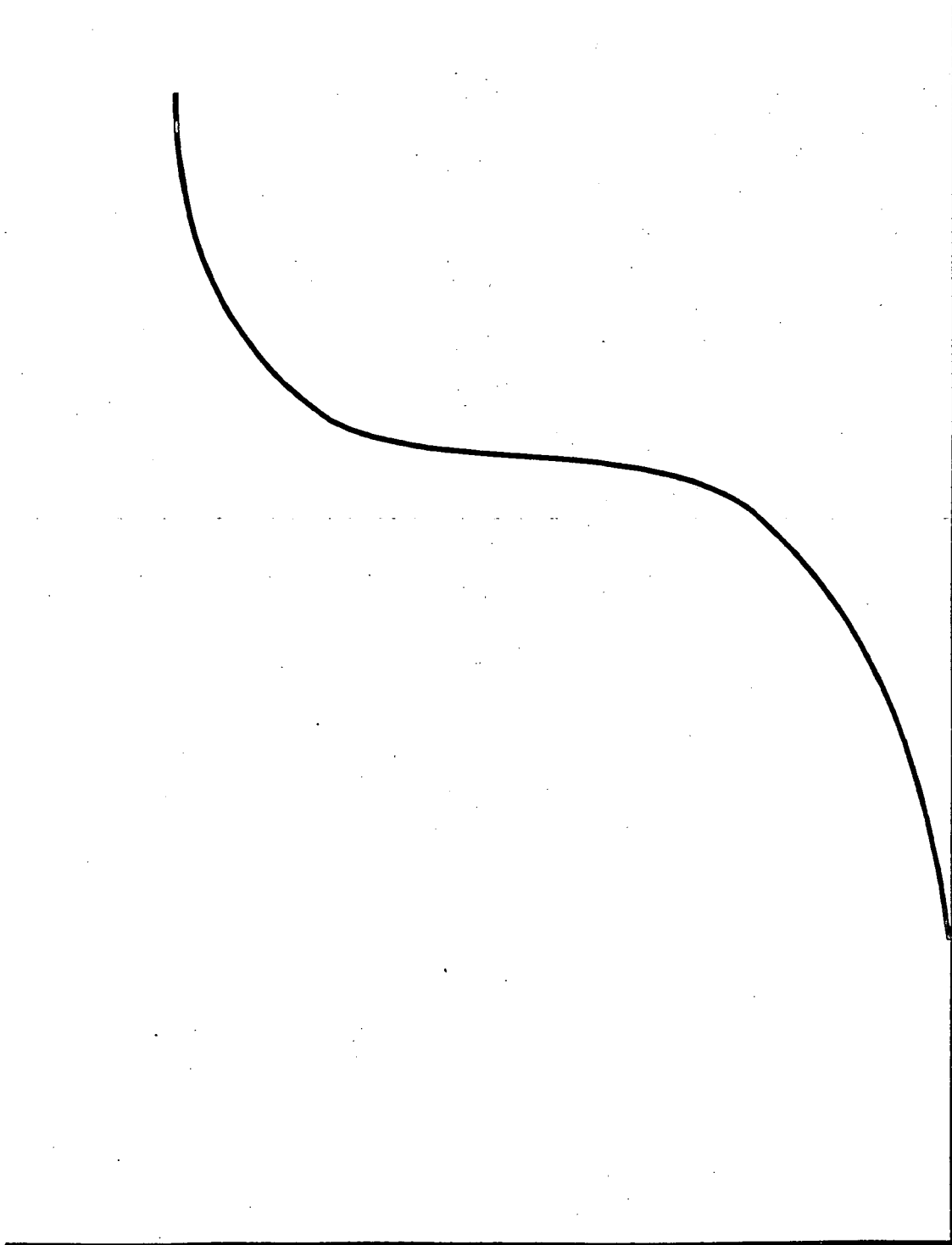
“ . . . the dose which an operator can, for prolonged period of time, tolerate without suffering injury.”

- 50 That, of course, led them to believe that they had a curvilinear threshold response, that is, if you kept the dose below a certain value,  $1/100$  of the erythema dose in 30 days, you wouldn't get a burn. This curve also says that there are some people that wouldn't get burns if they had  $1/10$  of an erythema dose in 30 days. Others would be even higher. One of the characteristics of a threshold curve is that it really is a curve which reflects individual variability in terms of effects and dose. The important concept is that there is a threshold and a threshold had been observed in many of the industrial chemicals of their day. They had every reason to believe that they were working with a threshold effect with radiation and if they kept below their erythema limit, they would be protected.

50

Incidence

Dose



2P

51 All though the 1920s, there had been attempts to standardize a way in which to characterize the radiation field. As we have seen, the erythema dose was the first. However, people were looking for a physical measurement. The International Congress of Radiology formed the ICRP, then known as the Advisory Committee on X Ray and Radium Measurements, in its congress in 1926. They reported to the next Congress that they had defined the roentgen. This standardized a quantity for everyone to see. The difficulty with the definition is that it is physically a difficult measurement because it is essentially a definition for what happens in air. Any time you try to make a measurement in air you surround that air with a chamber or with a detector and you are no longer measuring what happens in air, but you measure what happens in that detector and how its secondary emissions caused different things to happen. There have been a number of attempts to measure this but, in fact, the roentgen is a historical term and it is not used in the S.I. system. However, it will still often appear in manuscripts and particularly in those prepared several years ago. For our purposes, for X- and gamma rays there are no differences between a roentgen and rad.

6L

52

**1931**  
**1934**

NCRP recommendation

0.1 R/day

ICRP recommendation

0.2 R/day

55 Perhaps one of the more fortuitous things to happen was that we had a limit of 0.1  $\mu\text{gm}$  of radium in the body in 1941. The late Merrill Eisenbud had made quite a case out the fact that we had 0.1 R/day and 0.1  $\mu\text{gm}$  of radium, one a limit for external radiation and one a limit for internal radiation, at a time when we were beginning the Manhattan Project. It is hard to imagine what might have happened if these two standards were not in place. The radium data came from the radium dial painters throughout the country who had been tipping their brushes in order to get a fine point on their radium-laden bristles to paint the time pieces. In addition, there had been a scurrilous case in which a wealthy playboy had died of an excessive ingestion of Radiothor, a radium-bearing elixir, which was supposed to bring him health and happiness. Instead, he died of radiation poisoning. All avidly reported in the popular press.

1941

Limit of .1  $\mu\text{gm}$  Ra in the body

56 At that time, Robley Evans was at University of California. The people at the health department in California did not want Southern California to be known as a place where a man died of drinking Radiothor, so they hired Robley Evans to look into the issue. Robley went on to MIT. Actually, it was Robley Evans who made the suggestion that there be the 0.1  $\mu$ gm of radium limitation. It was interesting to look at how this came about. Robley looked at 27 cases of ingestion among the dial painters and he saw that there were seven cases where there was no effect. Among these women, the ingestion was estimated to be less than .5  $\mu$ gm of radium. Twenty cases where there was 1.2-23  $\mu$ gm showed various effects. Some of these were just a change in bone density while others were necrosis of the jaw or bones, serious debilitating injuries. The U.S. Advisory Committee on X-ray and Radium Safety (the NCRP today) adopted its 0.1  $\mu$ gm and noted that this value was chosen by the Committee as a value they would not mind their wives and children being subject to.

## .1 $\mu\text{gm}$ Ra

Robley Evans

27 cases

7 cases  $< .5 \mu\text{gm}$  - no effect

20 cases  $1.2 - 23 \mu\text{gm}$  - various effects

Advisory Committee adopted  $0.1 \mu\text{gm}$  as the limit

- 57 I think that one of the less heralded groups of people that had an enormous impact on radiation protection after the war were those who were part of the Tripartite Conferences. They pulled together all of the research work which had gone on throughout the United States, Canada, and the United Kingdom during the war time, studies in metabolism, depth dose, RBE, ecological movement, bioassay. An enormous contribution from all of these programs was brought to these Committee meetings in order to see to it that all the best information from the war effort went into the radiation protection recommendations for the future.

**1948 - 1953**

**Tripartite Conferences**

The United States, Canada, and the United Kingdom reviewed wartime data for application to radiation protection.

58 It is rather staggering to realize the foresight that this Committee had as they looked at the data. They understood that in order to develop maximum permissible concentrations for various nuclides, they had to assume a Reference Man. They had to assume a given breathing rate. They had to assume a given body size, as well as getting as much information on metabolism as they possibly could. Reference Man was a major contribution from this organization. Next, the idea of a depth dose. They recognized early on that they were dealing with more than just 200 KW X ray -- which the depth dose pattern was one in which half of the dose was at 5 cm. They were dealing with high-energy X rays in which, in fact, the dose could be higher inside the body than it was outside the body. They knew that they weren't going to get half-value layer of 5 cm in tissue. What this meant to them was, if people had been protected earlier by the erythema limit, then in order to be protected for the future, the dose had to be no greater than half of what it was at the surface. The erythema dose was a surface dose measurement, in essence, and the organs at 5 cm -- bone marrow and other organs -- obviously had about half as much radiation, that being the depth dose for soft X rays. Now, however, with cobalt-60 and high-energy accelerators, they no longer were protected by a surface dose limit and had to have a dose limit, not only at the surface, but also at depth in the body. As seen earlier, this group also pulled together the information on RBE for neutrons and high-energy radiations. They came to the point of recommending values for dose limits based on their experience. Much of the material which this Committee gathered, at least conceptually, is in our radiation protection programs.

## TRIPARTITE CONFERENCES

- Reference Man
- MPCs for  $^3\text{H}$ ,  $^{14}\text{C}$ ,  $^{24}\text{Na}$ ,  $^{32}\text{P}$ ,  $^{35}\text{S}$ ,  $^{41}\text{A}$ ,  $^{60}\text{Co}$ ,  $^{90}\text{Sr}$ ,  $^{131}\text{I}$ ,  $^{210}\text{Po}$ ,  $^{226}\text{Ra}$ ,  $^{\text{Nat}}\text{U}$ ,  $^{233}\text{U}$ ,  $^{234}\text{Th}$ , and  $^{239}\text{Pu}$
- Depth Dose (organs at 5 cm)
- RBE
- Suggested 0.3 rep (rem)/wk at 5 cm  
and 1.5 rep (rem)/wk at 7 mg/cm<sup>2</sup> (skin)

- 59 The Tripartite recommendations reached the formative stage in NCRP, ICRP, and AEC during the period of 1949-54, in which they picked up many of their recommendations. I will have you note particularly the relationship between the limit for the skin and the limit for the organs within the body, that is, 600 mrem and 300 mrem.

1949 - 1954

NCRP	}	300 mrem/wk
ICRP		(blood forming organs, gonads, and lens of the eye)
AEC		600 mrem/wk in the skin

- 61 All though the period shortly following the Second World War, the atmospheric weapons testing program, which led to low-level exposures throughout the world, caused a number of individual scientists to complain bitterly about the impact this was having on the population. The assumptions that they were making were based primarily on the work of Mueller who had demonstrated that at high doses, there was a linear relationship between dose and effect for the fruit fly.

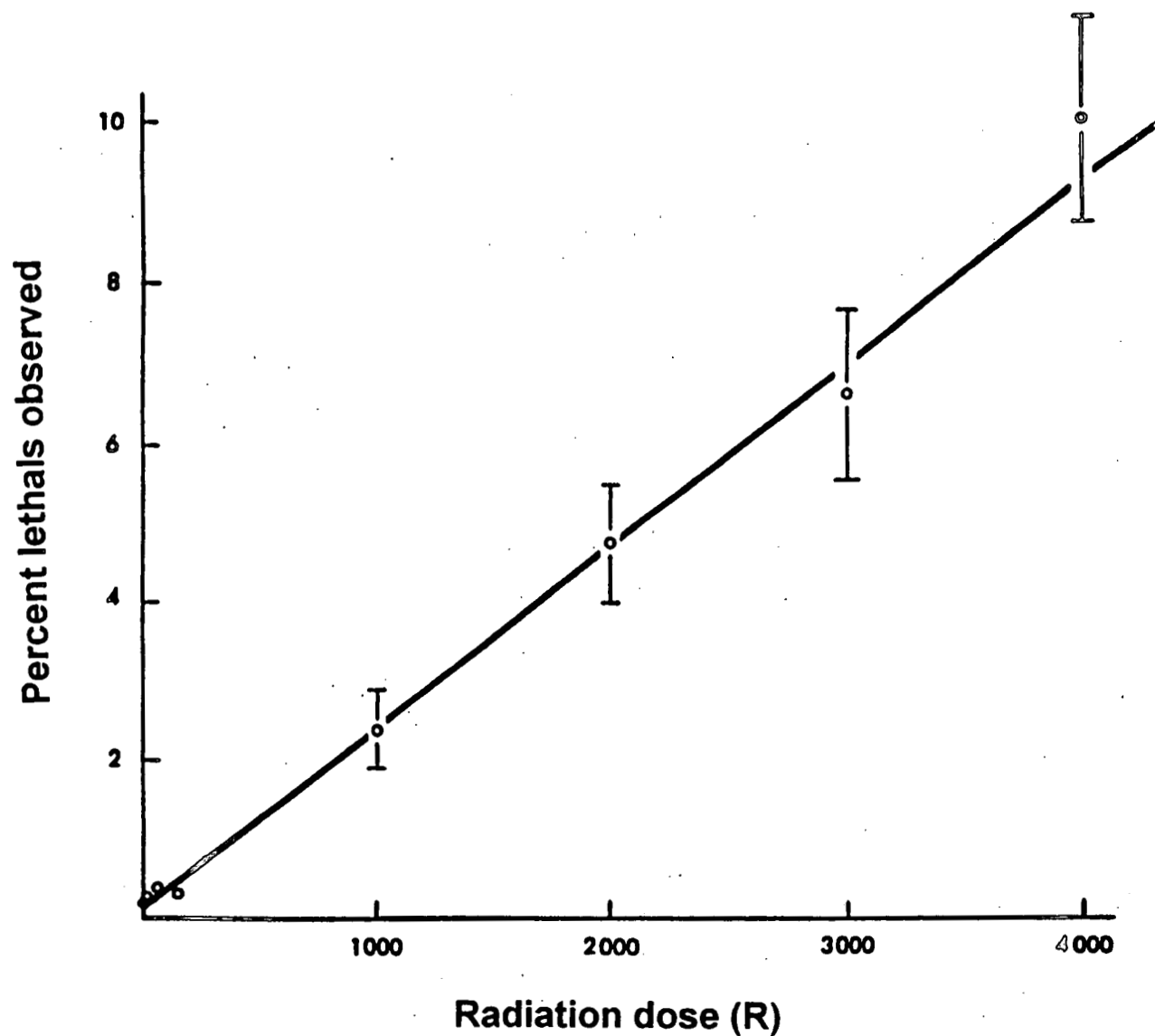
**LINEARITY**  
**(NO THRESHOLD)**

61

91

- 62 This is data from Spencer and Stern of 1948 showing you the strength of that data, that is, that for the mutation rate in the *Drosophila* fruit fly, they could show a very linear relationship. Note particularly, the very high doses. This data and its presentation were to have a profound effect on radiation protection over the next 30 years.

**Relationship between mutation rate and radiation dose to  
*Drosophila* spermatozoa. (Data from Spencer and Stern, 1948)**



A. P. Casarett

94

- 63 This linear relationship became established first as indicated in genetics. In fact, it was genetics which drove the recommendations of the National Academy of Sciences National Research Council and the Medical Research Council of the UK, both of which were formed by 1954, following the Bravo Nuclear Weapon test. I was intrigued with these reports, primarily the Academy report. In trying to find out whether it was just genetics, I turned to Gene Cronkite who was Chairman of the Somatic Committee of the Academy, who told me that they knew there was excess leukemia in the early radiologists, but they didn't have any dose information and it seemed that the recommendation of the genetics groups was certainly sufficient to protect everyone. Indeed, genetics became the basis for the recommendations. You notice also in these 1956 recommendations that there are age-related limits, 50 R to age 30, etc., 200 R lifetime, which changed the approach that had been taken to the old 0.1 R/day.

**1956****National Academy of Sciences, National Research Council**

Individual workers

50 R to age 30  
+50 R to age 40

Population

10 R to age 30

**Medical Research Council, UK**

Individual workers

50 R to age 30  
200 R lifetime

Population

< 6 R to age 30  
(2 times background)

96

64 By 1958, both the ICRP and NCRP were recommending  $(\text{age} - 18) \times 5$ , which, of course, was the NRC regulation, up until adoption of 10 CFR Part 20 (revised) in 1995. You notice that it applied to the whole body, blood-forming organs, and the gonads and that there was a rate limit as well of 3 rem for 13 weeks. This  $(\text{age} - 18) \times 5$  limit is simply the practical implementation of those National Academy of Sciences Medical Research Council numbers, essentially a way to ensure that over a lifetime the dose would be limited to about 200 rem, or to age 30 and again, to age 40, it would be no more than 50 rem. And so the  $(\text{age} - 18) \times 5$  is an annual limit that was adopted in accordance with the recommendations of those committees. I don't want you to forget, however, as we look at that question, that it was only on genetics that the number arose, thought to be protective for leukemia. Notice also the limit of 30 rem for the skin and 15 rem for individual organs.

# 1957 - 1958

## Annual Limits

### NCRP

(Age - 18) 5 rem  
3 rem/13 wks

30 rem

15 rem

### ICRP

5 rem/30 yr

Whole body, blood-forming  
organs, and gonads

Skin

Individual organs

Population

66 UNSCEAR, which you remember was created at the same time as the Medical Research Council and the BEIR Committee of 1954, had been following the work of the Atomic Bomb Casualty Commission very closely. The Atomic Bomb Casualty Commission, now called the Radiation Effects Research Foundation in Japan, is jointly funded by the United States and Japan. It has been following, on an individual basis, the people who were exposed to the bombings of Hiroshima and Nagasaki. During the early days, the only disease that had been seen was Leukemia, which had been expected primarily because of the experience with the radiologists. The table does demonstrate that there was a steady increase in the incidence of solid tumors in the Japanese survivors during the period 1962-77. How was all of this to be taken into account?

## UNSCEAR

1962	Leukemia ( $1-2 \text{ y}^{-1} 10^{-6} \text{ rad}^{-1}$ ) other malignancies noted
1964	Leukemia: other malignancy = 1:1
1972	Leukemia: other malignancy = 1:2
1977	Leukemia: other malignancy = 1:5 thyroid, breast, lung, bone (brain, salivary glands, stomach, G.I. tract, bladder, lymphoid tissue, and liver)

- 150
- 67 During the middle to late 1970s, the ICRP recognized that information on risk was becoming available in a manner that might allow them to use risk in their dose limits decision-making. They published this material in 1977, which changed radiation protection dramatically. For the first time, the principles and limits were grounded in a scientific approach to risk estimation, not simply a new approach to evaluating the erythema dose limits. The Commission particularly noted that it had to have a different way of treating stochastic and nonstochastic effects. It was, of course, the adoption of ICRP Publication 26 that drove the revision to NRC's 10 CFR Part 20.

# **RISK-BASED DOSE LIMITATION**

ICRP Publication 26 (1977)  
Basis for NRC 10 CFR Part 20 (1995)

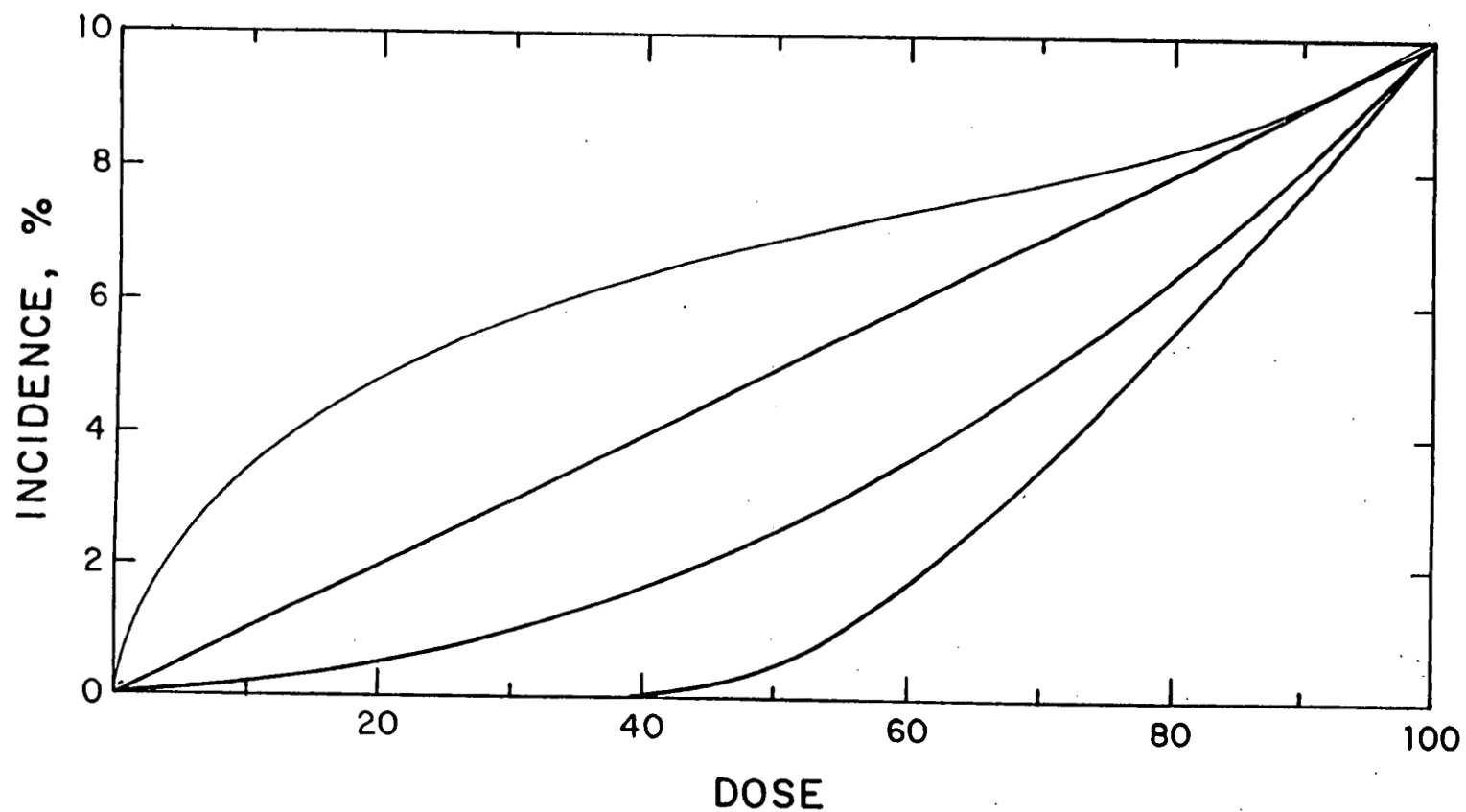
Stochastic effects  
Nonstochastic effects

- 102
- 68 Perhaps the most significant single element of these new recommendations was a careful presentation of the information gained from the Japanese survivors and the way in which it was extrapolated for use in radiation protection. Picking up on the radiobiology from 30 years earlier, the Commission emphasized that the shape of the curve should respond to a  $\alpha D + \beta D^2$  relationship. That is, a relationship which incorporates a term for a single hit event ( $\alpha$ ) and one which serves for multiple hits ( $\beta$ ). The top curve is superlinear, i.e., it would predict a larger number of cancer cases per unit dose at low doses than at high doses. The second curve is a pure linear extrapolation. The third curve is an  $\alpha D + \beta D^2$ , allowing for recovery and repair at low doses and low-dose rates. The fourth curve has a threshold.

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# DOSE RESPONSE MODELS



- 194
- 69 The linear extrapolation at low doses and dose rates led inevitably to the system of protection which involved three concepts. If there is to be some level of risk for some level of dose, every use of ionizing radiation has to be justified. If there is some level of risk with every level of dose, then you must be willing to spend money, at least enough money to offset the detriment that you are causing, to reduce those exposures. For those times when you can't reduce them on an economic basis, we still need a series of dose limits which protects the individual from optimization, in which the benefit concerns others, but for which he receives the exposure. At this stage, I should point out that ALARA and optimization are synonymous as far as the NCRP is concerned. ICRP uses optimization, NCRP uses ALARA, but they mean the same thing.

**Therefore, radiation protection requires...**

**Justification**

**Optimization (ALARA)**

**Dose Limits**

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- 79 Perhaps no other branch of science has been more important, nor more controversial to radiation protection, particularly in terms of assessing the risk, than the whole field of epidemiology and biostatistics. I, therefore, will try to introduce this subject in some detail and give you examples of important topics related to our understanding of risk that have arisen from these epidemiological studies. During this introductory session, I will be drawing very heavily on three important sources of information. The first is an excellent presentation made by Dr. Elizabeth Cardis at the IRPA International Congress in 1996; the second, is an excellent paper by Dr. John Boice in the NCRP Proceedings of the 32nd Annual Meeting of the National Council on Radiation Protection and Measurements, Implication on New Data on Radiation Cancer Risks, Proceedings 18, 1977; and third, is an excellent monograph by Anders and Ahlbom, 1979.

# EPIDEMIOLOGY

- Introduction to the Principles
- Cohort Studies
- Case Control Studies
- Ecological Studies

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80 As we begin this review, we need to stop for a minute to look at the two general objectives. One of them is really a descriptive kind of measure. For instance, is malaria spreading from one region of South Africa to another? The second one, is it changing over time, is there more or less malaria in one country in a year, in 1996, than there was in 1986. This second one is analytical and here, much more attention has to be paid to the individual and the individual's exposure if we are looking at the identification of what caused an increase in cancer, or malaria, or heart disease. These requirements are even great if we want to be able to quantify this in terms of risk per unit of exposure to whatever agent is under study.

## WHAT IS EPIDEMIOLOGY?

- Science aimed at studying the health of populations
  - descriptive:
    - geographical distribution
    - temporal trends
  - analytical:
    - identification of causes of diseases
    - quantification of effects

- 81 As mentioned at the beginning of this course, the definition you see here is relatively narrow, but as long as we all understand what it is we are talking about, we ought to be able to proceed without confusion. The terms cohort and incidence are fundamental to epidemiological studies and we will be returning quite frequently to those concepts. Notice that the incidences are the cases diagnosed over a fixed and stated time.

## DEFINITIONS

**Risk:** probability that, in a given time period (from time  $t_0$  to  $t_1$ ), a healthy subject becomes ill of the disease of interest, conditional on not having died from another cause in that period

**Cohort:** group of individuals, all disease-free at  $t_0$ , who are followed up to  $t_1$

**Incidence:** number of cases diagnosed in cohort between  $t_0$  to  $t_1$

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82 Causality is perhaps the root of our question. What caused the effect? My favorite example of causality is the suggestion that eating ice cream causes drowning, since one can correlate the increase in ice cream consumption with the number of drowning deaths.

# CAUSALITY IN EPIDEMIOLOGY

**Risk factor:** any factor related to the risk of disease

*Note:* the factor does not have to be necessary or sufficient, e.g

- individuals can get lung cancer even if they are not exposed to radon
- not all individuals exposed to radon will develop lung cancer

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83 It is through a list such as this that we can perhaps throw out my ice cream example. First of all, the temporal sequence. In cancer epidemiology, for instance, we know that it takes time for a cancer to develop after exposure and therefore, if the incidence of solid cancers shows a rapid increase shortly after exposure, there is some question, certainly, about the cause of the increase. In my ice cream story, this merely means that people can't drown until there is a lot of ice cream and they are going to keep on drowning after the ice cream sales remain elevated. That might even be true. The next item is reproducibility which means that you've got to be able to demonstrate that the study that you have performed isn't an artifact of some temporal nature, and that it can be reproduced by other investigators or with other groups in order to demonstrate that it is true. Strength of statistical association means that if you don't have enough people, you don't have enough disease, you don't have a long enough study time, you won't be able to accurately discern whether there is a cause or not. The dose-response relationship is very important in terms of establishing causality. If I could show that the amount of ice cream sales increases each week during the summer and that the drowning is in accordance with that increase, that is, there are fewer drownings in the first week than there are in the second and as the weather gets hot I see an increase in ice cream sales and therefore I can see an increase in drowning. For the Japanese survivors, this is very important. There is probably no single thing that is more helpful than establishing causality. Effective removal of the risk factor, this probably would be the fundamental flaw that would come up in my ice cream story. That is, if we took away ice cream in the community, sold no more ice cream or frozen yogurt for a period of a summer, and saw that the drowning level didn't change, then my causality picture falls apart. This is probably the best way to prove that I am incorrect in my assumption. And the last is similar, biological plausibility. When the BEIR Committees do their studies they make a big point of this. You really have to be able to show that it makes sense in terms of biology. A good example of this was a study by Gardner et al. in the U.K. in which he suggested that exposure of the father to high doses of radiation led to leukemia in his offspring. Many of the geneticists that looked at this didn't believe that there was any way in which you could establish that this was a biologically plausible effect.

## CRITERIA FOR CAUSALITY

- temporal sequence
- reproducibility
- strength of statistical association
- dose-response relationship
- effect of removal of risk factor
- biological plausibility

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- 84 Here we are reminded that you can't just take the number of individuals with a disease, for instance, breast cancer, and say therefore, we know that living near a nuclear power station causes breast cancer. In this case, we have the situation indicated in the first bullet. It can't be emphasized enough that if you are going to try to establish an association between the cause and effect, you need information on each individual, whether the disease exists or not, on the exposure, and whether or not there are confounding effects. Are there other ways that the individual has operated in his lifestyle that could cause this cancer? Clearly, smoking and lung cancer are one of the most important confounding factors.

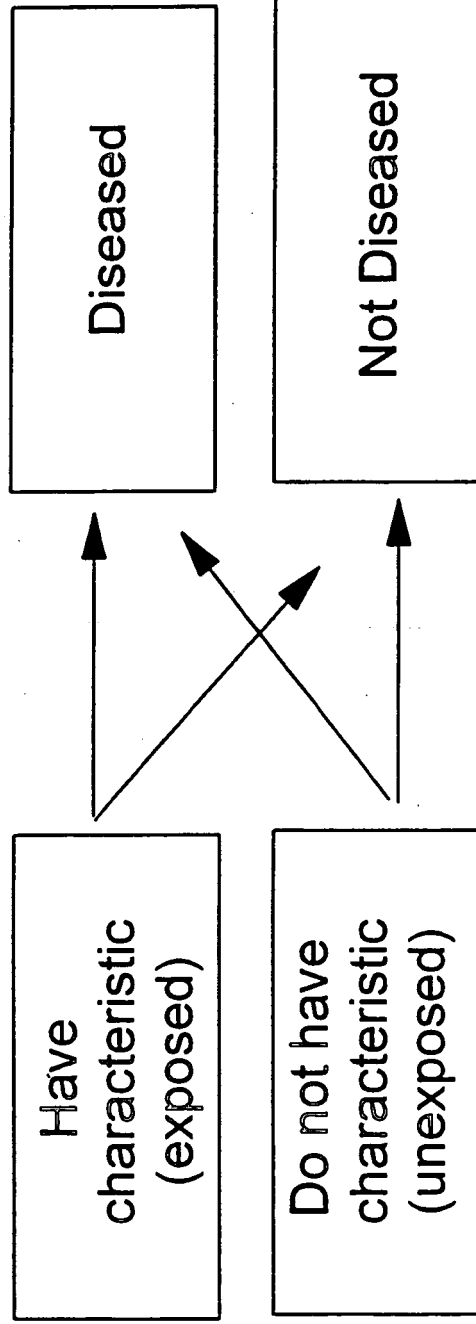
## TEST OF AN ASSOCIATION

- Cannot rely only on cases:
  - *exposure may be very frequent among cases, but also among persons who are not ill.*
- Need information on individuals:
  - *disease*
  - *exposure*
  - *confounding factors*

- 86 Let's look in detail at what we mean by a cohort study in epidemiology. In this case, we look at a group of people who have been exposed. We look at a similar group of people, trying to be as complete in terms of lifestyle, economics, ethnic background, age distribution, sex distribution -- a population very similar to those who were exposed, but who did not have the exposure. In the cohort study, what we do is begin with this group of people at the time our study begins  $T_0$  and we follow them up to some time,  $T_1$ . During that time, we characterize all those who have become diseased. Let's suppose it is our smoking study. There are going to be people with lung cancer who do smoke, that is the exposed group, and there are going to be a large number of people who have lung cancer who did not smoke. Therefore, we must characterize each of these in terms of the outcome, in terms of lung cancer. For those who are exposed, many of them may have lung cancer, but many will not. It is sorting this out that is the stuff of an analytical epidemiological study. The way that that's done is to look at the number of cases per person-years in the exposed population and divide it by the number of cases per person-year in the nonexposed population.

# COHORT STUDIES

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- Calculation of the relative risk:

	Exposed	Non-exposed
Number of cases	$d_1$	$d_0$
Person-years	$PY_1$	$PY_0$

$$RR = \{d_1/PY_1\}/\{d_0/PY_0\}$$

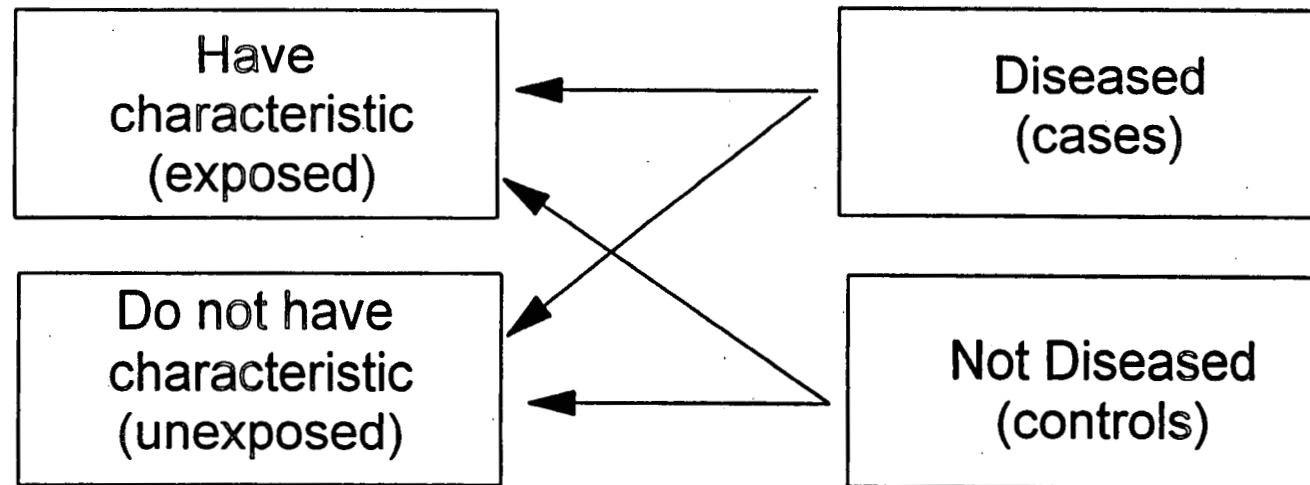
- 93 Now that we have learned about the cohort studies, we can review the advantages of such a study. The first item under the first bullet is fundamental to the question that we had before us, that such a study can estimate the incidence in the exposed and nonexposed and relative risk. The advantage to having the exposure known before the disease is that it greatly reduces the chance for bias and people remembering or not remembering whether or not they have been exposed. The disadvantage is, which is very, very important for our studies, is the tremendously long follow up. In fact, in order to really understand the impact of cancer on a population exposed to radiation, you have to follow them over their entire lifetime. That is almost an impossible task. The other disadvantage is that the outcome is rare. If you are looking for a specific cancer, trying to find those cancer cases can be very rare in terms of its arising in the population you have selected. Because you are dealing with a large number of individuals, obtaining information for all the subjects is very, very costly.

# ADVANTAGES AND DISADVANTAGES OF COHORT STUDIES

- Advantages:
  - *can estimate incidence in exposed and non-exposed and relative risk*
  - *exposure level known before disease occurrence*
- Disadvantages (e.g., cancer and radiation studies)
  - *very long follow up*
  - *rare outcome*
  - *cost of obtaining all information for all subjects*

- 122
- 94 Here we can demonstrate the difference between the cohort study and the case control study. As you remember in the cohort study, we started with people who had been exposed and those who had not been exposed, and followed them forward to determine whether they became diseased. In the case control study, we actually start with a group of people who have been diseased, who have, say, lung cancer if we were looking at smoking, and we would then take another group of people with similar characteristics who did not have the disease, that is, the same age distribution, ethnic background, and socioeconomic status, whatever we thought might be important, particularly smoking history. Then we would determine for all individuals in both groups whether they had been exposed. By using the odds ratio, we can determine an increase in the relative risk. We do this by using a 2 x 2 table in which we have cases (a) who are exposed, and (b) not exposed, and the controls, those who were exposed and those who were not exposed. We take the ratio of the number of exposed individuals to the number of nonexposed individuals and divide by the ratio of the number of cases to the number of the controls, i.e.,  $a/c + b/d$  or  $ad/bc$ .

# CASE CONTROL STUDIES



- Calculation of the odds ratio:

	Exposed	Non-exposed
Cases	a	b
Controls	c	d

$$OR = a d / b c$$

*OR : odds ratio - estimate of RR*

*can look at dose-response in a similar way*

- 95 The choice of study subjects is very important in a case control study because they have to be representative of those getting the disease. For instance, if you only used the cases given to you by an HMO rather than the cases which might come out of a university medical center or a private medical center in a large city, you would get very different results. So you need to know all of the cases of whatever effect or disease you are investigating in the population under study. The controls have to be very carefully selected as well. As mentioned before, age and sex are very important to establish that your controls have the same structure as the people who have the diseases. This one can be quite confusing. They must be representative of the population of interest with respect to exposure, that is, not restricted to those who are not exposed. For instance, in a large population like the Japanese, you can actually be more comfortable about making sure that you have good controls if it is the population, say the O-1 rad group who were the people in Japan who were very similar to the rest of the Japanese who got exposed at that time. You don't necessarily have to restrict it to those people who weren't exposed.

## CHOICE OF STUDY SUBJECTS

- Cases:
  - *must be representative of those getting the disease in the population of interest*  
i.e., all cases of leukemia in a given time period among Chernobyl liquidators
- Controls:
  - *must be comparable to cases for factors other than exposure*  
i.e., age, sex, ... - this is obtained by matching or stratification at design stage
  - *must be representative of the population of interest with respect to exposure*  
i.e., not restricted to those who are not exposed if range in population

- 96 The overriding advantage in a case control study is that it is much faster and much cheaper because you don't have to wait for the occurrence of the cancer, you go to the cancer cases and backtrack from there. You can collect a lot more information for the same amount of money spent. The disadvantage is that you really can't estimate the incidence in the exposed and nonexposed and there is a real possibility of bias. If someone has lung cancer, the individual may not remember, or want to remember, if they were a smoker or a nonsmoker. This can cause a possibility of bias and this is true of many other studies of disease. Of course, you are only studying the disease that you chose, for instance, lung cancer. So, a big disadvantage is that it's only one disease at a time.

# ADVANTAGES AND DISADVANTAGES OF CASE-CONTROL STUDIES

- Advantages
  - *much quicker and cheaper: no need for decades of follow-up of very large populations*
  - *allows collection of detailed information of much reduced number of subjects and more quality control*
- Disadvantages
  - *cannot estimate incidence in exposed and non-exposed*
  - *exposure level not known before disease occurrence -- possibility of bias*
  - *study only one disease at a time*

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- 97 This is an interesting presentation prepared by Dr. Cardis which looks at the question of confounding, that is, is there something that is happening in that population that can confuse the results? So she sets up a population in which there are 5,000 smokers, 5,000 nonsmokers, and then says, all right, let's determine whether their exposure to electromagnetic fields causes lung cancer. Now, since there are many more smokers than nonsmokers in this population, when she puts together her study, she finds out that of the study population, 1,500 were smokers and 500 were nonsmokers. Of the not exposed, 3,500 were smokers and 4,500 were nonsmokers. Again, the total is 5,000 in each category. Now she decides to check whether or not there is an effect on these people from exposure to magnetic fields. She looks at the diseased population and finds out that 1,300 have been exposed to electromagnetic field, 3,700 have not. For the people who do not have the disease, she finds that 700 were exposed to the electromagnetic radiation and 4,300 were not exposed. So when she calculates the odds ratio, as we did before, she gets 2.15 and she gets a simple figure of a sigma uncertainty 1.95 - 2.4. Saying to herself, yes, in this case I think that I've established that there is an exposure-related effect from electromagnetic radiation, but perhaps smoking has confounded the results.

## DISTRIBUTION OF SUBJECTS BETWEEN CASES AND CONTROLS, AND SMOKERS AND NONSMOKERS

	Diseased	Not Diseased
Smokers	4,000	1,000
Nonsmokers	1,000	4,000
Total	5,000	5,000

With Confounding

Case 1: Distribution of the subjects between exposed and nonexposed to electromagnetic fields, and smokers and nonsmokers

	Smokers	Nonsmokers
Exposed	1,500	500
Not Exposed	3,500	4,500
Total	5,000	5,000

Case 1: Distribution of the subjects between cases and controls, and exposed and non-exposed to electromagnetic fields

	Diseased	Not Diseased	Total
Exposed	1,300	700	2,000
Not Exposed	3,700	4,300	8,000
Total	5,000	5,000	

$$OR = 2.15 (1.95 - 2.4)$$

- 98 Let's try to fix this problem so that smoking does not have such an impact. This time the people are stratified as we see in case two. Here, we see the exposed population now consists of 1,000 smokers and 1,000 nonsmokers, essentially keeping smoking out of the equation. Of the nonexposed, 4,000 are smokers and 4,000 are nonsmokers. So she still has the same number of smokers, 5,000 as well as the same number of nonsmokers. But she has stratified this so that the number of smokers will not have an impact on her estimate of the risk. When she does this, she finds an odds ratio of 1. That is, essentially there is no effect of electromagnetic radiation on that cancer. But you can see how misled you would have been if you hadn't corrected for smoking and the difficulty is that those confounding factors are not always as clear as they are with lung cancer and smoking.

**DISTRIBUTION OF SUBJECTS BETWEEN CASES AND CONTROLS,  
AND SMOKERS AND NONSMOKERS**

	Diseased	Not Diseased	Without Confounding
Smokers	4,000	1,000	
Nonsmokers	1,000	4,000	
Total	5,000	5,000	

Case 2: Distribution of the subjects between exposed and nonexposed to electromagnetic fields, and smokers and nonsmokers.

	Smokers	Nonsmokers	Total
Exposed	1,000	1,000	2,000
Not Exposed	4,000	4,000	8,000
Total	5,000	5,000	

Case 2: Distribution of the subjects between cases and controls and exposed and nonexposed to electromagnetic fields

	Diseased	Not diseased	Total
Exposed	1,000	1,000	2,000
Not Exposed	4,000	4,000	8,000
Total	5,000	5,000	

$$OR = 1 (.09 - 1.1)$$

99 &amp; 100

We often hear in discussions of epidemiological studies, particularly those that are proposed, that the power of the study is not sufficient. What this really means is that if I have a small population of people and a very low incidence of that cancer expected as a result of the exposure, I can pretty much tell before I even start whether or not it's likely that I'll be able to see the effect. There is a mild fallacy in that the understanding of the expected depends upon our present knowledge of risk. We must be careful that we understand that as we look at the power of the study. Precision, on the other hand, has to do with having done the study -- what is the level of confidence that we have around the risk estimate? How tight is the confidence interval? Is it close enough for us to take this information into account as we try to develop our risk estimate?

# POWER AND PRECISION IN EPIDEMIOLOGY

- Power:
  - *probability of detecting an effect when it exists*
- Precision
  - *width of confidence interval around risk estimate*

# POWER AND PRECISION IN EPIDEMIOLOGY

## (cont.)

Depend on:

- *characteristics of the study*
  - number of subjects
  - length of follow-up (for cancer, particularly)
  - errors in dosimetry (for dose-response analyses)
- *characteristics of the association under study*
  - frequency of disease in the absence of exposure
  - frequency and distribution of exposure in population
  - size of risk associated with exposure
  - importance of potential confounding factors

- 136
- 101 The last category that Elizabeth discussed in her presentation was geographical and ecological studies. As indicated much earlier, these are good for comparisons of group data across a geographical area with different average exposures to the cancer of interest, i.e., lung cancer and smoking. It can look at changes in terms of time. The advantages are that it is extraordinarily inexpensive and quick. You just go to cancer registries and choose whatever you would like to use in terms of establishing the exposure and as a result, it is used widely to monitor population health.

## GEOGRAPHICAL / ECOLOGICAL STUDIES

- Comparisons of grouped data:
  - *across geographical areas with different average exposures to the factor of interest*
  - *over time*
- Advantages:
  - *inexpensive and quick, if appropriate population data is routinely available (cancer registries, etc.)*
  - *tool for monitoring population health*

- 138
- 102 The major disadvantage is that it can't detect small risks. For instance, for the atomic bomb survivors, if we had used the geographical study instead of the cohort study, we'd find that the relative risk would be 1.09 and, in fact, our confidence in the data is much greater than that because we have information that is stratified at high levels of dose.

## GEOGRAPHICAL / ECOLOGICAL STUDIES (cont'd)

- Disadvantages
  - *low power to detect small risks*
    - e.g., atomic bomb survivors
      - *only 9% of all cancer deaths to 1990 are attributable to radiation exposure ...*
      - *very difficult to detect such an increase (RR = 1.09) if compared mortality between regions exposed as a result of the bombing to comparable non-exposed regions*

- 140
- 103 Another disadvantage is that there are a number of biases which can creep into a geographical study, particularly the ecological fallacy, which is the failure of a group data to reflect the individual levels of associations. For instance, I may know that the ratio of cancer and smoking and radon exposure are all intermingled, but I don't know the information on the individual, which can lead to the ecological fallacy. So again, it is most important for monitoring populations and generating some hypotheses, but it cannot establish whether there was a causal relationship between some exposure and some effect.

# GEOGRAPHICAL / ECOLOGICAL STUDIES (cont'd)

- Disadvantages (cont'd)
  - *Subject to a number of potential biases, in particular the “**ecological fallacy**”:*
    - the failure of group level data to properly reflect individual level associations
- Conclusion:
  - *Useful for monitoring populations and generating hypotheses*
  - *Cannot establish causality*






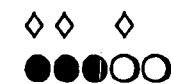
142

104 Elizabeth presented a very interesting example of the ecological fallacy. Looking at geographical zones a, b, and c, she postulates that 20% of these people wore hats in zone a, 40% wore hats in zone b, and 60% wore hats in zone c. She wants to ask whether there is any relationship between head colds and wearing hats versus not wearing hats. The percent who wear hats and the percent who have head colds in zones a, b, and c were the same, 20%, 40%, and 60%. Let's look at two situations, situation 1 and situation 2. In situation 1, we have a total of one person who wears a hat and who has a cold. There are also five individuals who did not wear a hat and who had a cold. These are represented by the black circles without the diamond over them, the diamond being the hat. For those who did not have a cold, represented by the open circles, there are 5. We set up our odds ratio, essentially  $1 \times 4 + 5 \times 5$ , which gives an odds ratio of 0.16. Let's look at situation 2. Here she arbitrarily changed the number of people who are wearing hats and who have a cold. In this case, you will notice in geographic zone a, one person has a cold but wears a hat. In geographic zone b, two people have colds and both wear hats. And, in geographic zone c, three people have colds two of which wear hats and one person who did not have a cold wears a hat. I now set out my table, again, for case 2. When I do that, I get  $5 \times 8 = 40 + 1 \times 1$  and the odds ratio is 40. You can see that nothing changed here. There were still hats and colds, but because no one can correlate whether or not a person with a cold did or did not wear a hat, there is no way to establish whether the odds ratio is 40 or 0.16.

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# AN EXAMPLE OF ECOLOGICAL FALLACY

Geographic Zone	Percent Who Wear Hats	Percent with Head Colds
A	20%	20%
B	40%	40%
C	60%	60%

Geographic Zone	Situation 1	Situation 2
A		
B		
C		

Odds-ratios for case 1(a)

a

	Cold	No Cold
Hat	1	5
No Hat	5	4

OR = 0.16

Odds-ratio for case 2(b)

b

	Cold	No Cold
Hat	5	1
No Hat	1	8

OR = 40

105 Up to this point, we have been talking about some concepts important to understanding epidemiological studies. Now what I'd like to do is to take you through some of the studies which have been performed and are important as we move toward an estimate of risk from radiation. Take these in the same order of cohort, case control, and ecological studies.

# **EPIDEMIOLOGICAL STUDIES OF IMPORTANCE FOR RISK ESTIMATION**

- **Cohort**
- **Case Control**
- **Ecologic**

106 Before we begin this analysis, it is very valuable to look at a presentation made by John Boice in his NCRP paper. Here he is essentially asking, what is the intrinsic quality of these studies as a function of the type of study and, what is their susceptibility to bias? The highest would be experimental, but almost impossible for us to do. You might do some in medicine where you might do a study on a drug or a procedure, but for general purposes, it is almost impossible. But if you do them, then the intrinsic quality is extraordinarily high because you can control all of the variables and the susceptibility to bias is extremely low. The cohort studies are the next in terms of the hierarchy. Cohort studies are, in fact, the strongest studies that we really can have for our general activities and these are both prospective and retrospective. Case control studies have somewhat less intrinsic quality and have considerably more susceptibility to bias. Those with the lowest intrinsic quality are the ecological studies which have the most susceptibility to bias.

**Table 1. Types of Epidemiologic Studies**

Type	Intrinsic Quality	Susceptibility to Bias
Experimental	Highest	Least
Cohort (Follow-up)		
Prospective		
Retrospective		
Case-control		
Ecologic (Geographic)		
	Lowest	Most

107 Boice has listed here in a table which I took from the NCRP Proceedings 18 the cohort studies for radiation-exposed populations. I think the most important study is that of the Japanese survivors. It is the study on which we base most of our radiation risk. It is an extremely long-term study of the Japanese survivors. It has followed each of those survivors forward in time, has worked very hard at establishing individual dosimetry, that is, the dose to each individual, and has carefully worked on ascertainment of death certificates. Recently, it has been expanded to look not just at mortality but also at morbidity, that is, the incidence of cancer. The advantage here is that incidence data is inherently more reliable than death certificate data. Perhaps the first of the important early studies was that of the radium dial painters done by Rowland et al. You will recall that it was a follow-on to the work that Robley Evans had done. This was important because the radium dial painters ingested very large quantities of radium and developed high rates of bone cancer. As we will see later, Lubin has reviewed a very large series of eleven important comprehensive studies from the miners and he has shown quite clearly that there is lung cancer associated with that exposure. Perhaps the most recent study that has worldwide recognition is that of Elizabeth Cardis et al, who combined studies from nuclear workers in the United States, the United Kingdom, and other countries to see if she could get some estimate of risk. Although the power of the study is extraordinarily weak, there is a suggestion that the leukemia risk estimates are not very different from that which are used in the risk estimates by NCRP and ICRP.

## **Table 2. Cohort (follow-up) Studies of Radiation-exposed Populations**

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### **Atomic Bomb**

Japanese Survivors (Pierce et al., 1996; Thompson et al., 1994)

Marshall Islanders (Conard, 1984; Robbins and Adams, 1989)

### **Workers**

Radiologists (Lewis, 1963; Smith and Doll, 1981; Wang et al., 1990a)

Miners (Lubin et al., 1994; 1995)

Radium Dial Painters (Rowland et al., 1978)

Nuclear Workers (Cardis et al., 1995; Gilbert et al., 1993)

Radiation Technologists (Boice et al., 1992c; 1995)

- 150
- 108 The medical studies have been particularly helpful in terms of individual organ estimates. These studies on ankylosing spondylitis serve primarily as a reference point for the Japanese survivor data, and at one time they were very close. However, they are somewhat divergent at the present time. The ankylosing spondylitis studies were of people with arthritis of the spine that had been irradiated in the United Kingdom. The difficulty is that the dose to the bone was very high while the dose to the other tissues was somewhat lower. Again, the dose rate was high.

The tinea capitis studies of Ron et al. have been very helpful both in terms of good data for brain cancer, thyroid cancer, and for skin cancer of the head and neck. The skin cancer is particularly interesting because it demonstrated this question of whether the ultraviolet light is a co-carcinogen with radiation.

Large numbers of studies shown in this overhead reflect the fact that there has been a lot of work on breast cancer which has been shown quite clearly in some studies and not in others. The data is beginning to suggest that the reason for this is that age at exposure is extraordinarily important. In fact, Land et al. have made the point that not only is it age, it's also age at first pregnancy which is a heavily controlling factor. To give you an example of the sorts of things that one gets from these individual cohort studies, the thymic enlargement studies indicated that breast cancer in childhood didn't manifest itself until 40 years later. This is also true in the studies with Botha et al. which showed that radiating children with Hodgkin's disease resulted in breast cancer at a much later age, that is, in adulthood. The Thoratrast studies are of great interest in terms of liver cancer. Thoratrast is a colloidal solution of thorium dioxide which is used as a contrast medium. The Danish incidence series now suggests that a liver cancer incidence of 55%, 50 years post-injection, but they are still not clear whether this is a chemical or radiological issue.

**Table 2. Cohort (follow-up) Studies of Radiation-exposed Populations (cont'd)**

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**Medical**

Ankylosing Spondylitis (Weiss et al., 1994; 1995)  
Tinea Capitis (Ron et al., 1988; 1989, 1991)  
Thymic Enlargement (Hildreth et al., 1989; Shore et al., 1985)  
Benign Breast Disease (Shore et al., 1986; Mattson et al., 1993)  
Benign Gynecologic Disease (Inskip et al., 1990; 1993)  
Tuberculosis (Boice et al., 1991; Miller et al., 1989)  
I-131 (Hall et al., 1992; 1996)  
Thorotrast (Andersson et al., 1992; 1994)  
Cervical Cancer (Boice et al., 1987; 1988)  
Hodgkin's Disease (Hancock et al., 1993; van Leeuwen et al., 1995; Bhatia et al., 1996)  
Breast Cancer (Boice et al., 1992a; Storm et al, 1992; Inskip et al., 1994)  
Childhood Cancer (Tucker et al., 1984; 1987; 1991; Olsen et al., 1993; Hawkins et al., 1996)

- 109 Boice did do a review of the power of the Japanese life span study to help us understand how likely it is that that study is going to provide a result which is meaningful. Boice put down the relative risk numbers from UNSCEAR from the lifespan study showing that when you have a dose on the order of 200 rem or 2 Gy, then the ability to detect the risk is extraordinarily high. At 100 rem or 1 Gy, it is still medium and positive. It is still available at 25 rem, but even though you might have some trend analysis that suggests attributable cancer at 10 rem and at 1 rem, the fact is that the ability to test the risk is becoming diminishingly small as you get to 10 rem.

**Table 3. Ability of Epidemiologic Methods to Detect Cancer Risks Following Acute Radiation Exposure to Low LET Radiation, i.e., LSS**

<b>Dose (Gy)</b>	<b>Probable RR (UNSCEAR, 1994)</b>	<b>Ability to Detect Risk</b>
2.00	1.8 - 2.0	High
1.00	1.4 - 1.5	Medium
0.25	1.10 - 1.20	Low
0.10	1.05	Very Low
0.01	1.005	Improbable

154

110 The case control studies have also been very important in terms of the evidence that we've gotten particularly from the prenatal X rays. The work done by Alice Stewart starting in 1955 and extended to the present time has undergone extensive revision and quality enhancement and it now suggests that there is an excess risk of leukemia in those who were exposed prenatally to X rays. We'll come back to that a little later when we discuss the fetus. Another series in this case control group of the breast studies, the Curtis study, for instance, looked at those who had received therapy for breast cancer and compared the leukemia outcomes in those who had been treated with surgery, those who had been treated with radiation, and those who had been treated with chemicals. It was found that the incidence of leukemia was doubled over the surgical patients for those who had received the radiation but it was actually increased by a factor of 10 for those who had received chemotherapy. In fact, however, most of these studies on breast cancer, these case control studies, give risk estimates that are somewhat lower than the lifespan study. For the environment, the important one is the indoor radon, where there has been a lot of controversy recently, where there has been a major study with Lubin and Boice in 1997. We'll look at some of that material a little bit later as well.

## **Table 4. Case-Control Studies to Evaluate Radiation Exposures**

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### **Medical**

Prenatal X-ray (UNSCEAR, 1994; Stewart et al., 1958; Monson and MacMahon, 1984; Harvey et al., 1985)

Cervical Cancer (Boice et al., 1987; 1988)

Endometrial Cancer (Curtis et al., 1994)

Breast Cancer (Curtis et al., 1992; Inskip et al., 1994; Boice et al., 1992; Storm et al., 1992)

### **Environment**

Indoor Radon (Blot et al., 1990; Pershagen et al., 1994; Alavanja et al., 1994; Lubin and Boice, 1997)

156

111 Ecologic studies, and there have many such studies, were reviewed by the UNSCEAR. For example, there have been a number of studies of natural background, some in China and other parts of the world, and there is no evidence of increased risk but the power of the study actually wasn't capable of detecting it. The Chernobyl studies that are listed here are really just the summary of the studies that are going on now to look at thyroid cancer in children. Some of them have said yes, some have said no and just now they're beginning to do case control studies to see if they can sort some of this out. The nuclear facilities' studies are referring particularly to the study of clusters of leukemia cases around some of the nuclear power plants in the United Kingdom. Once these studies got under way, it soon became clear that there was no reason to correlate environmental pollution with these clusters. This goes back to something I mentioned earlier, that people get very confused that, in fact, leukemia must cluster if we think it is truly a stochastic phenomenon. If it's truly stochastic, it cannot be uniform and it must be random. And, if it's random, some of it has to be in clusters.

## **Table 5. Ecologic (Geographic) Studies of Radiation Exposure**

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### **Environment**

Natural Background (UNSCEAR, 1994)

Radon (Stidley and Samet, 1993; Cohen, 1993; Greenland, 1994)

Fallout

Weapons Testing (UNSCEAR, 1994)

Chernobyl (Kazakov et al., 1992; Mettler et al., 1992; Williams, 1994; Stsjazhko et al., 1995; Beebe, 1996; Karaoglou et al., 1996)

Nuclear Facilities (UNSCEAR, 1994; Forman et al., 1987; Jablon et al., 1991)

158

114 Important things to consider when evaluating epidemiological studies are these concerns that Boice presented. The first is the selection bias. You have to be very careful, particularly when studying worker populations to determine if they have been well treated in terms of their medical care, follow-up annual physicals, etc. which makes them unlikely to be as at higher risk from their exposure as someone who does not have that care. The reverse of that is when you are performing medical studies and the patients on whom you have made these studies are rather unhealthy, and they have a higher risk of getting cancer than the general population. Confounding is terribly important and as mentioned, smoking is the outstanding one but there are others. Part of the trouble in looking for given cancers in a population is that you have to be careful that you haven't instituted special screening techniques. An example might be the thyroid cancer in the Chernobyl regions where they might never have looked for nodules or cancer among that population. You have to be careful that you have not introduced a very important observational bias. The same is true of an interview bias. An interview bias means that once you know what you are looking for, your questioning intent is to elicit the answers that you are after. Recall bias is essentially a parent or a person who has the disease and they may have a much more vivid way of recalling what has happened in terms of the potential exposure. Chance is an important thing. I have said often that many people have confused random with uniform. In fact, if you didn't have clusters of leukemia, then you could not have it random. If it is random, it cannot be uniform. It is important to remember that bias can arise very easily. The other one is that if you are looking at twenty different cancers, then one of those is going to be out just by chance.

**Table 6. Epidemiologic Concerns**

<b>Sources of Error</b>	<b>Example</b>
1 - Selection bias	Healthy workers Unhealthy patients
2 - Confounding	Smoking
3 - Observation bias	Special screening
4 - Interview bias	Special probing
5 - Recall bias	Better memories
6 - Chance	Clusters Multiple comparisons

160

115 Now we begin the work at hand. We begin by seeing if we can establish with some high degree of certainty the risk per unit dose at high dose rates. That means, the lifespan study, primarily. We will also look at the projection model, since we have to project to the future for the Japanese because they haven't all died yet. From there, we will move on to our estimates of risk per unit dose at high dose rates.

## **RISK / DOSE AT HIGH DOSE RATES**

- **Lifespan Study**
- **Projection Model**
- **Risk/dose at High Dose Rates**

162

116 If I had only one overhead to show you in this course, this would be the one. This is the most recent data from the lifespan study -- clearly, the most important epidemiological study that we have for risk estimation. This is a cohort study of 86,572 people on whom both the dose and the outcome are known with a fairly high degree of reliability. You will recall in our discussion of ecological studies that if you were looking at the Japanese data here in front of us as a total, there is only a relative risk of about 1.09. As you recall from our previous discussions on reliability, this would put us in the category of very low probability of detecting an effect. However, if we divide these survivors into dose groups, that is, those who received 100-200 rem, we see that there are 1,608 of them. Of those 1,608, we would have expected to find 131, but instead, there were 215 cases of cancer in that group. That is an excess of 84 cases. That is not difficult to see in an epidemiological study. In that same group, we would have expected 4 cases of leukemia and instead there were 26, an excess of 22 cases. Within the group of 50-100 rem, there are 3,202. We would have expected 263 cases of solid cancer, but instead there were 336, an excess of 73 cases. Leukemia, again, expecting 7, but instead there were 33 cases. This is rather outstanding in a group of only 3,202 survivors. It is still quite detectable from 20-50 rem; 6,308 total, 555 cases expected, and instead there were 632, an excess of 77 cases. They would have expected 12 cases of leukemia, and instead there were 27, an excess of 15 cases. I think what this clearly demonstrates is that there is an elevation of cancer to people exposed to high dose rates of ionizing radiation. I would like to stop for a minute because many people talk about this as being high dose and high dose rate. The fact is that the categories even at the highest level, 100-200 rem, are the doses that we will permit under the 10 CFR Part 20 guidelines over a working lifetime, so that the doses aren't the question. The real question is the dose rate, the rate at which the radiation was delivered to these people.

# ESTIMATED NUMBER OF EXPECTED AND OBSERVED CANCER DEATHS BY CATEGORIES OF EXPOSURE

Dose (Rem)	Approx. Survivors	Solid Cancer		Leukemia	
		Expected	Excess	Expected	Excess
0 (<.5)	36,459	3,054	-42	64	9
.5 - 10	32,849	2,711	84	62	-3
10 - 20	5,467	485	19	11	0
20 - 50	6,308	555	77	12	15
50 - 100	3,202	263	73	7	16
100 - 200	1,608	131	84	4	22
> 200	679	44	39	2	28
<b>Total</b>	<b>86,572</b>	<b>7,243</b>	<b>335</b>	<b>162</b>	<b>87</b>

based on Preston

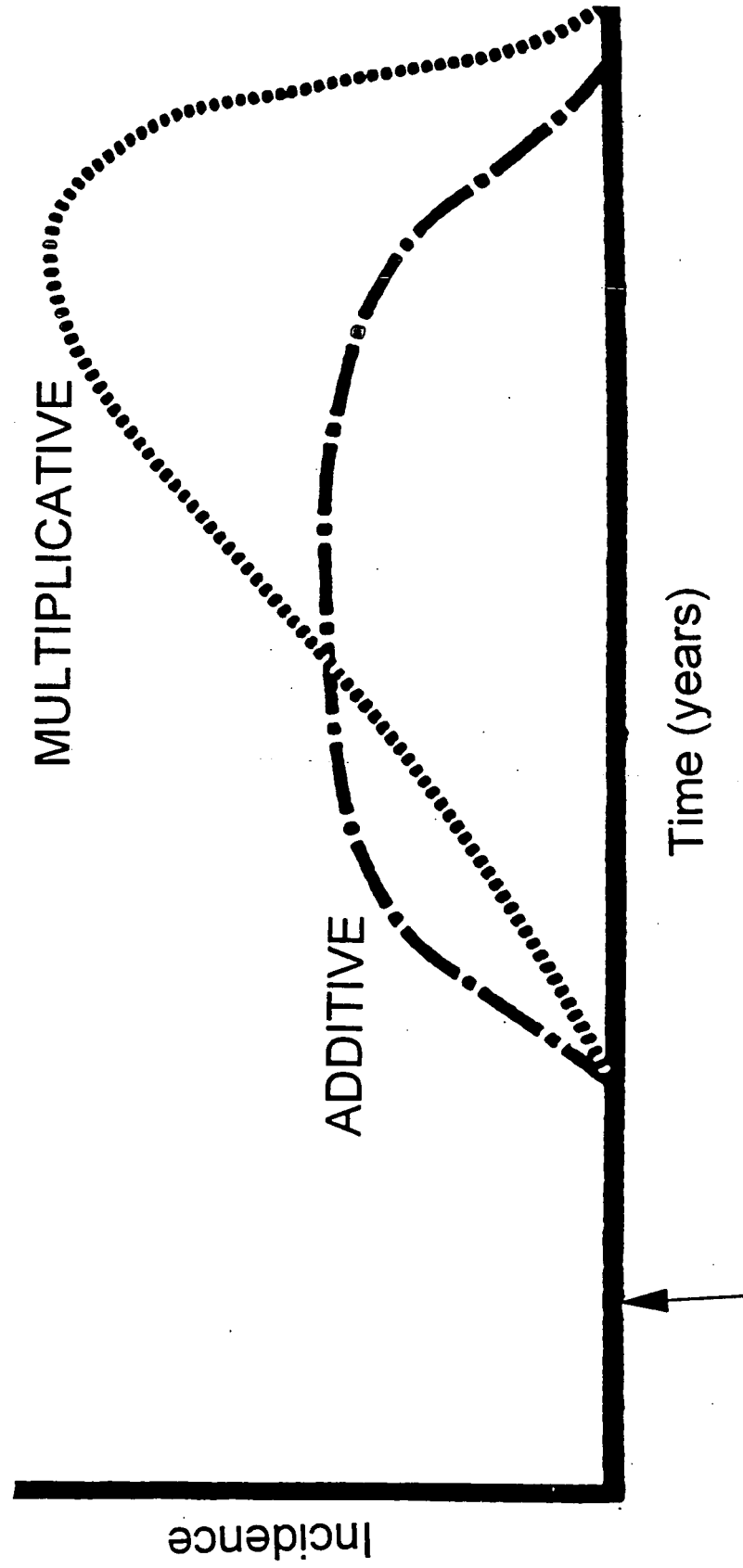
- 117 One of the areas of some uncertainty in our work has to do with projection models. The reason we have a projection model issue is that no population has ever been studied over their lifetime. We began studies of the Japanese in 1947. Some of those people were old, some young, some *in utero*. We certainly have not finished with those who were young or *in utero*. In fact, less than half of the Japanese survivors have died. How are we to establish then the level of risk over an entire lifetime for a radiation worker who is exposed at, say, age 18? How will we determine his risk per rem received at that time? We have two basic choices: an absolute risk model, which we saw earlier really means that we look at the population, what we have in terms of the risk that has already been shown to us by the people that have been exposed and have expressed that as cancer. We look at the pattern of incidence. Is there a latent period over which no cancer is expressed, and then a period of time in which it increased and further reaches a plateau, in time dropping off? We can estimate then some sort of risk for the future. Or, on the other hand, I can just assume, which I think is becoming more and more likely in terms of the biology of the system, that in time we would expect that what is happening is that we are simply amplifying the risk we have of cancer. That is, if cancer is a change in molecular structure of a DNA molecule and it goes through many stages, and in any stage in that process a change can happen due to ionizing radiation, then we are simply going to take whatever is the normal course of events and amplify it, increase it. That is really what the multiplicative model does. In fact, most people who have looked at all of these realize that we aren't working with a single cancer. We are working with a whole palette of cancers, all of which may have different patterns of the way in which the risk is expressed. One of the major problems in radiation protection is that we have to take averages and the multiplicative model is apparently the best one for us to use, although certainly not for leukemia.

## PROJECTION MODELS

- No human population has been studied for a full lifetime
- How do we go from observations in a fixed period of time to estimate lifetime risk?
- Additive model or multiplicative models

- 118 This gives you some concept of what these models look like. The incidence is given as a function of time, the arrow indicates that this is the point at which the exposure takes place. We see that there is no risk for the first five or six years for leukemia, and maybe for 10 to 15 years for solid cancers. The incidence of solid cancers doesn't start to rise for 15 or 16 years and then, it rises rather steeply under the additive model, up to some plateau where it remains, and then at some later time, it starts to go down primarily because the population is dying from other causes. Now let's try it with a multiplicative model. Here we see that the incidence at earlier times is somewhat less, but that the overall incidence is high, i.e., the area under the curve is greater. We are going to get an increase in the number of cancers. Just as important, we are shifting the time at which that cancer is likely to occur to later in time.

# PROJECTION MODELS



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120 This is derived from a table given in ICRP Publication 60 which had been prepared for the Commission by Dr. Arthur Upton. It gives us the cancer attributable to 1 Gy uniform whole-body exposure for the general population as estimated by various groups that have looked at this information. I'd like to show you a few things on this table. We deal first with the UNSCEAR over the years. Note that in 1977, UNSCEAR didn't even present a multiplicative model. UNSCEAR then brings us down to 1988 when they said that the probability of fatal cancer is about  $4 \times 10^{-2}/\text{Sv}$  or  $4 \times 10^{-4}/\text{rem}$ , an increase by about a factor of 2. Now, this change in the absolute risk came about because there were additional cancers over what would have had been projected but also there was a change in the dosimetry. A much more sophisticated dosimetry system was put in place in 1988. In 1988, as well, UNSCEAR believed that the multiplicative projection model was the better model to use. That would give us a value of 7.0 to 11.0% per Sv. This is not very different from the numbers that in NUREG 1991, which gives us a value of 11.2 as the upper value and a median something like 9.2. The fact is that pretty much everyone can say that there is a nominal estimate of the probability of death using a multiplicative model of about 10% per Sv.

## Excess Lifetime Mortality from all Cancer Attributable to 1 Gy Acute Uniform Whole-Body Low-LET Irradiation of the General Population<sup>(1)</sup>

Source of Estimate	Probability of Death ( $10^{-2}$ )	
	Additive Risk Projection Model	Multiplicative Risk Projection Model
BEIR I, 1972	1.2	6.2
UNSCEAR, 1977	2.5	-
BEIR III, 1980	0.8 - 2.5	2.3 - 5.0
NUREG, 1991	3.5 - 9.2 - 11.2	
UNSCEAR, 1988	4.0 <sup>(2)</sup> - 5.0 <sup>(3)</sup>	7.0 <sup>(3)</sup> - 11.0 <sup>(2)</sup>
BEIR V, 1990	-	8.85 <sup>(4,5,6)</sup>

(1) Population of Japan

(2) Estimate based on age-specific coefficients of probability.

(3) Estimate based on constant (age averaged) coefficient of probability.

(4) U.S. population - adjusted to high dose using values from Table 8.

(5) Modified multiplicative model.

(6) "Low dose" leukemia component multiplied by 2.

- 170
- 122 Now we must take the information we have at these high dose rates and apply it to our radiation protection situation in which normally our exposures are at very low dose rates. This is the area of most of the controversy.

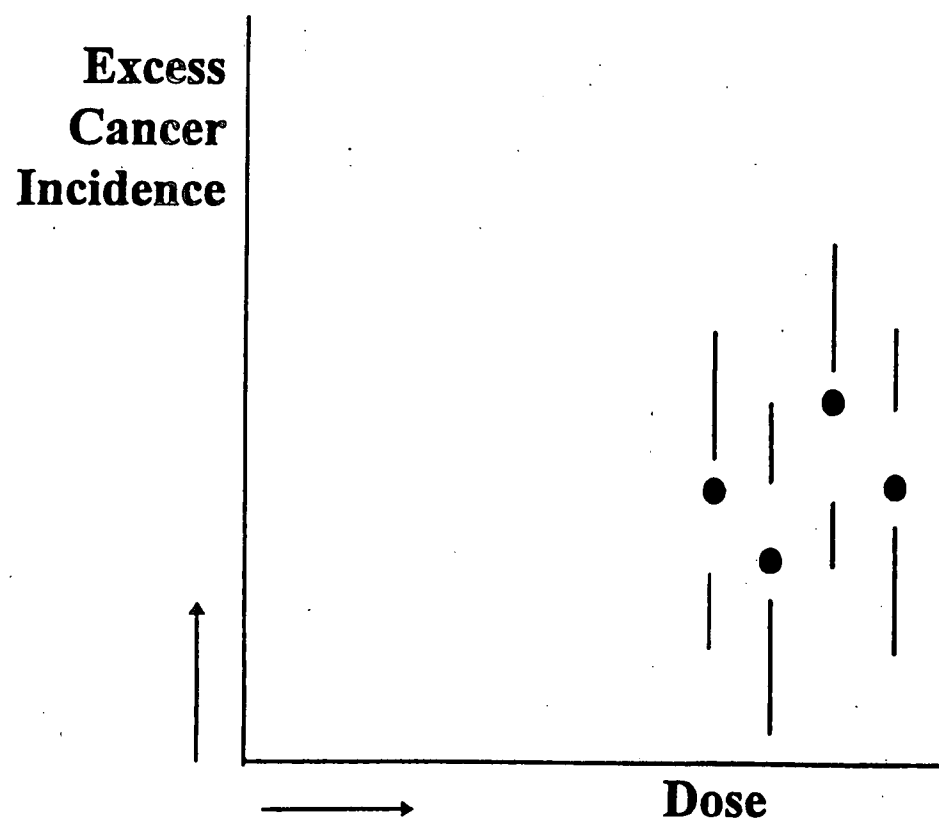
# **HIGH DOSE RATE EXPERIENCE APPLIED TO LOW DOSE RATE RISK ESTIMATES**

- Threshold
- Linear
- Hormesis
- DREF Value

172

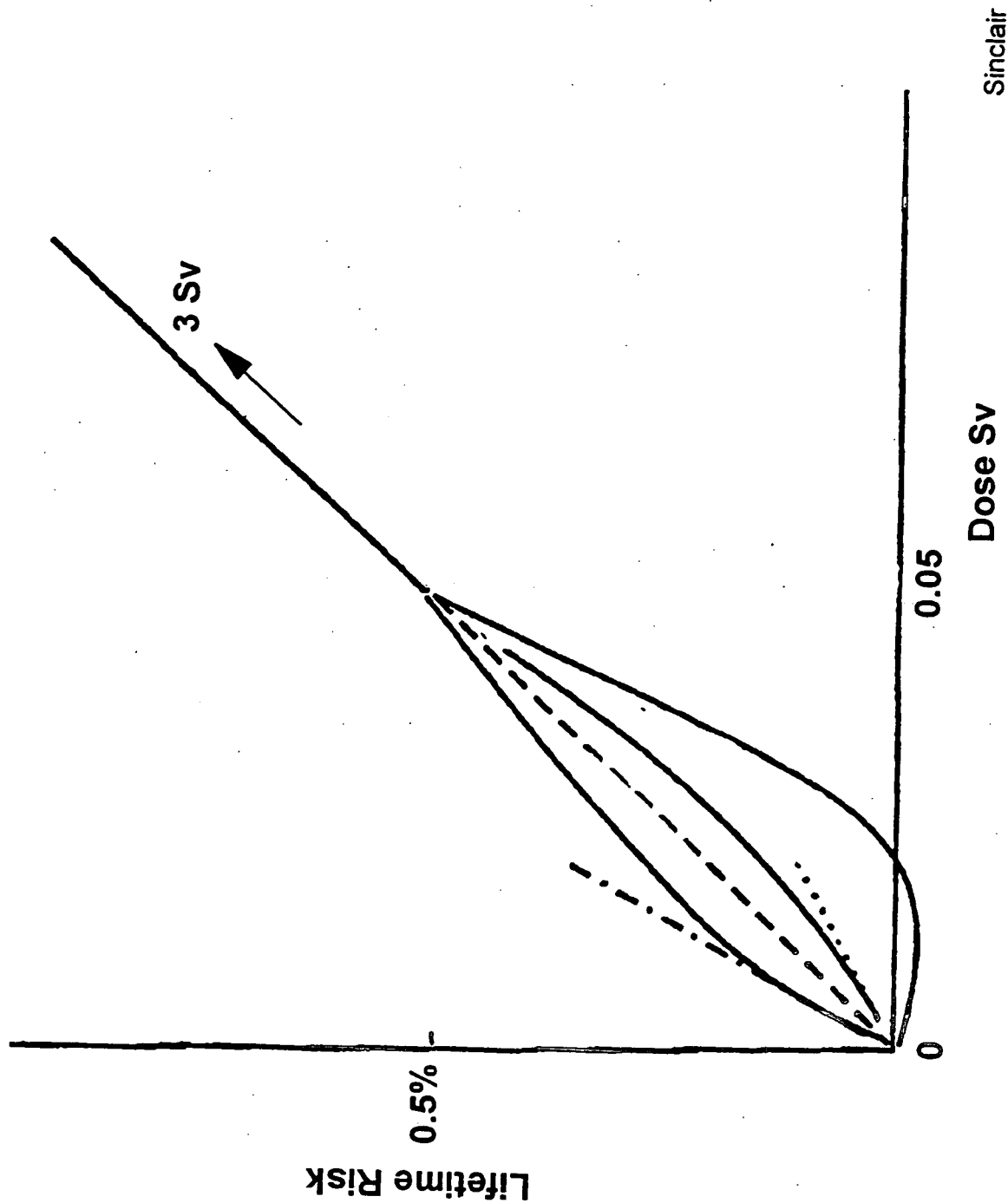
123 Here, pictured in a cartoon fashion, is the Japanese data. I do this only to let you know that from these data points alone, I cannot derive the relationship I need to get me to the very low dose and dose rates in the area of the arrows. As a result, I have to have a model. I may eventually bring that data down perhaps by another factor of 10, but the fact remains that I cannot get to extremely low dose rates and low doses with epidemiological data. I have to have a model. I've got to look at basic radiobiological data, the best information I can get from animal data and I have to make a judgment at what we do at low doses and dose rates.

# DOSE RESPONSE MODELS



124 Here is a sampling of the kinds of models that you can have. First of all, there is a super linear one, that is the top curve. That merely says that as you go down in dose, the number of cases per unit dose may actually increase. The dashed line, the middle, is a pure linear response, the kind we saw with *Drosophila* in the early 1930s, which we go right through the origin. The top dashed line is a simple extension of the slope of the super linear curve as it reaches some sort of a linear component. Just below the major dotted line is a linear quadratic, the  $\alpha D + \beta D^2$  we talked about back when we were looking at Publication 26. It means that there is a linear portion,  $\alpha D$  and a quadratic  $\beta D^2$  portion of the curve. The bottom curve is more interesting. This is what you would get if there is a hormetic effect. That is, as you come down in dose, not only is there a threshold where you would not expect an effect below this dose, but you think it is beneficial at low dose and dose rate. We will come back to that a little later as well.

# DOSE RESPONSE MODELS

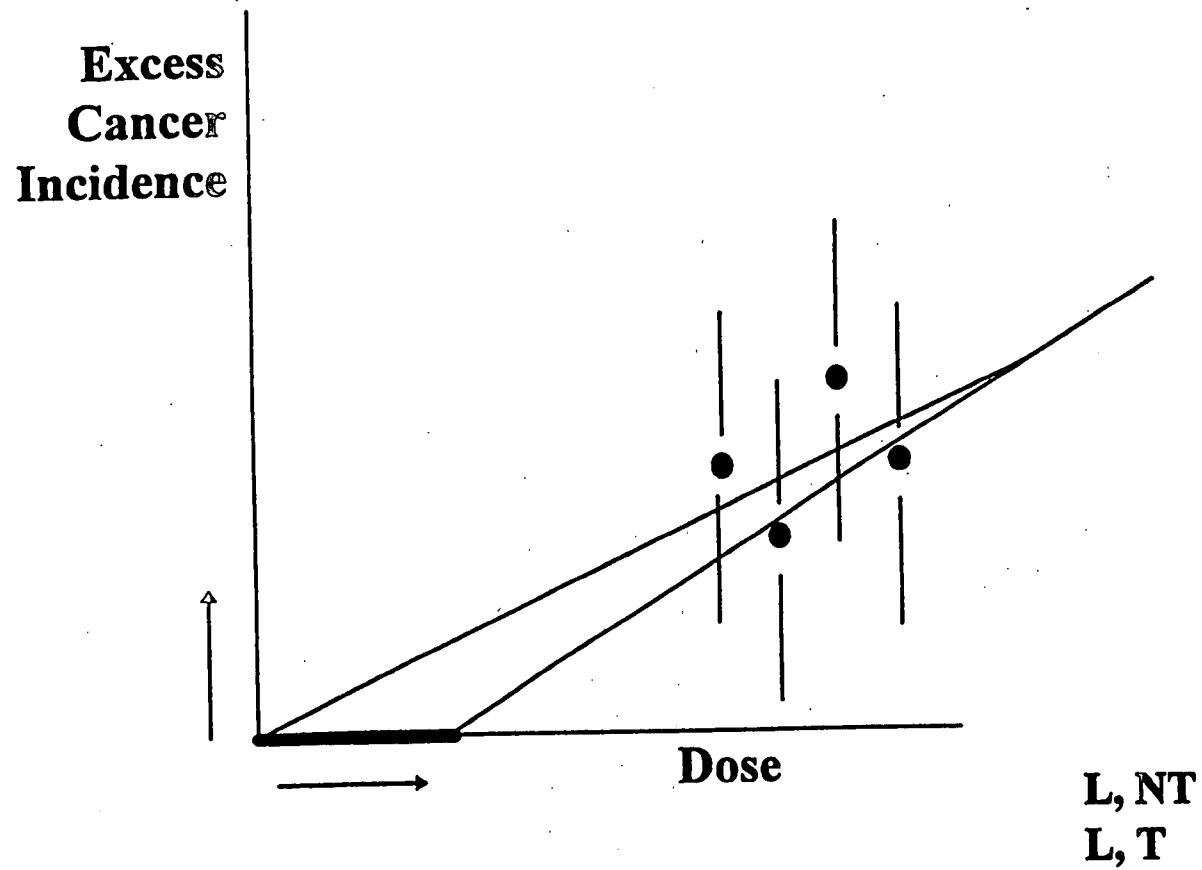


- 125 This shows us two important concepts, again, in cartoon fashion. Through those data points, you can have a linear threshold model or a linear nonthreshold model. This is true on almost every shaped curve. The idea of whether there is a threshold is somewhat independent of the shape of the curve. We see that we have a choice between a threshold and a nonthreshold model.

177

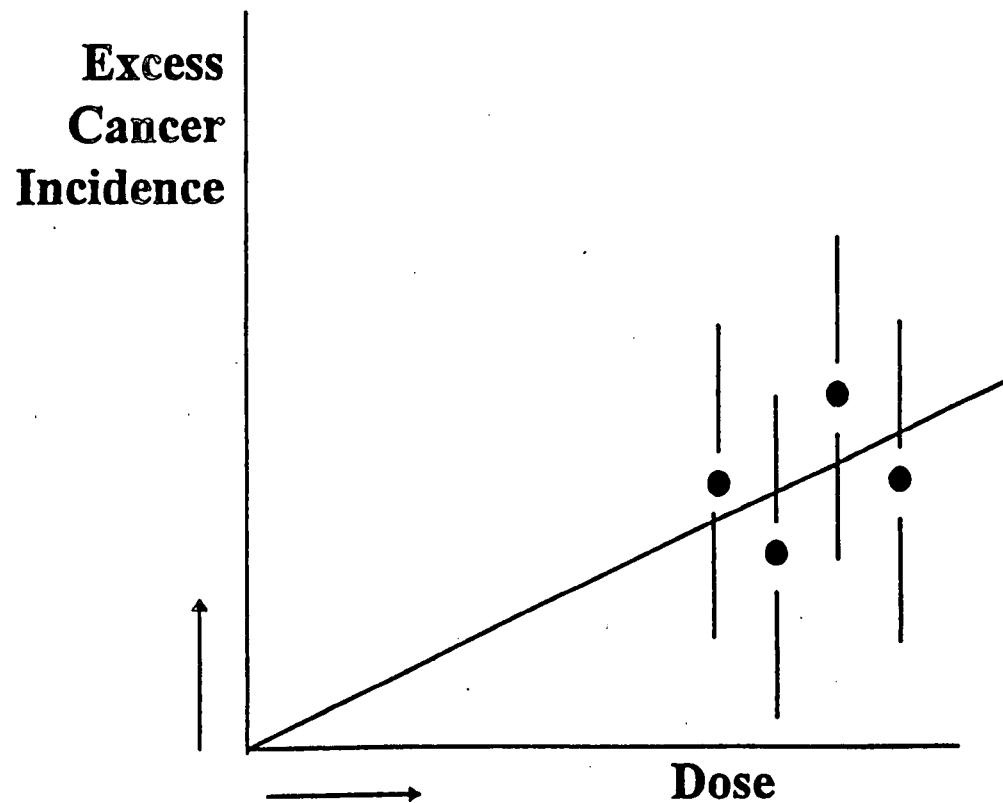
125

# DOSE RESPONSE MODELS



- 127 Let's go back to a pure linear response and ask if that makes any sense. This is a model which suggests that at some slope, you return from the doses what happens at high doses down through the origin.

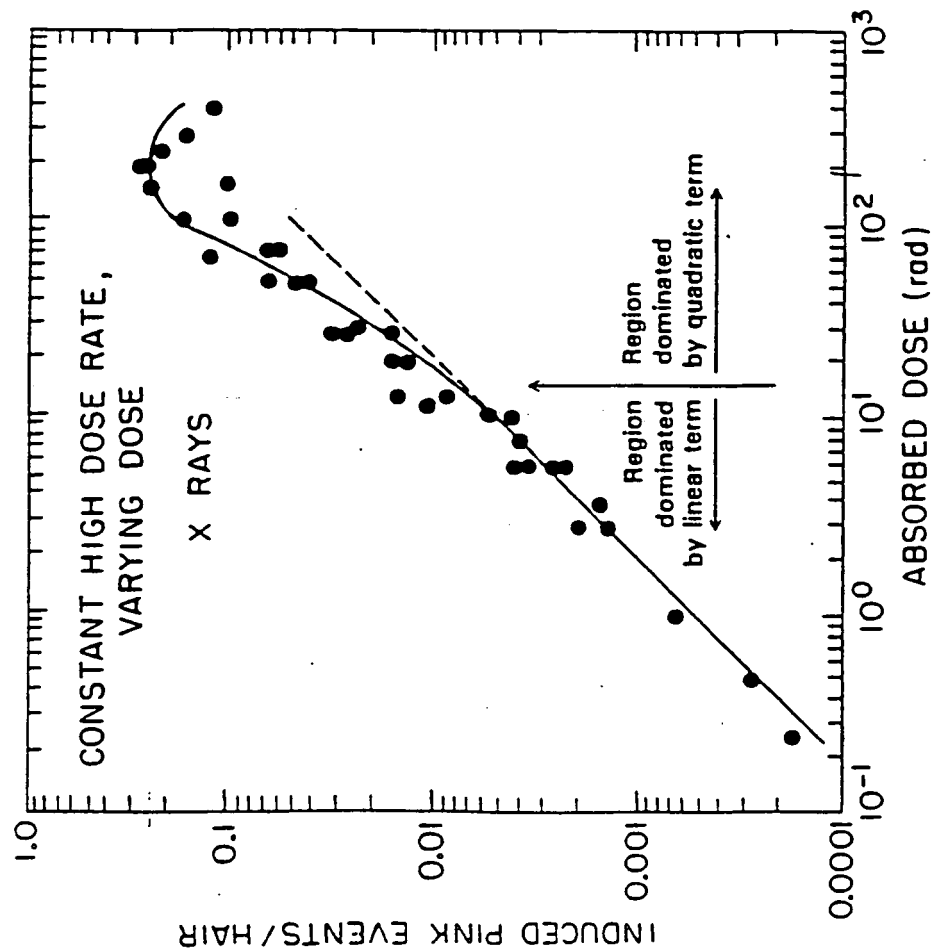
# DOSE RESPONSE MODELS



- 128 Perhaps the best evidence for a linear response of a biological system is about as far removed from the human being as you can be. That is the X-ray dose response for pink mutations in *Tradescantia*. You may recall that *Tradescantia* was one of the plants that some of the Japanese activists used around the power stations. In fact, it is an exquisite dosimetry system. Vic Bond, Harold Rossi, and I used the data from Harold Sparrow's experiments with this material to demonstrate that there was a difference between X rays and gamma rays. What is intriguing about this data is that you can come down in absorbed dose to extraordinarily low doses with the linear function. You'll notice that the lowest dose point is something like 200 mrad.

# LINEAR RESPONSE

128

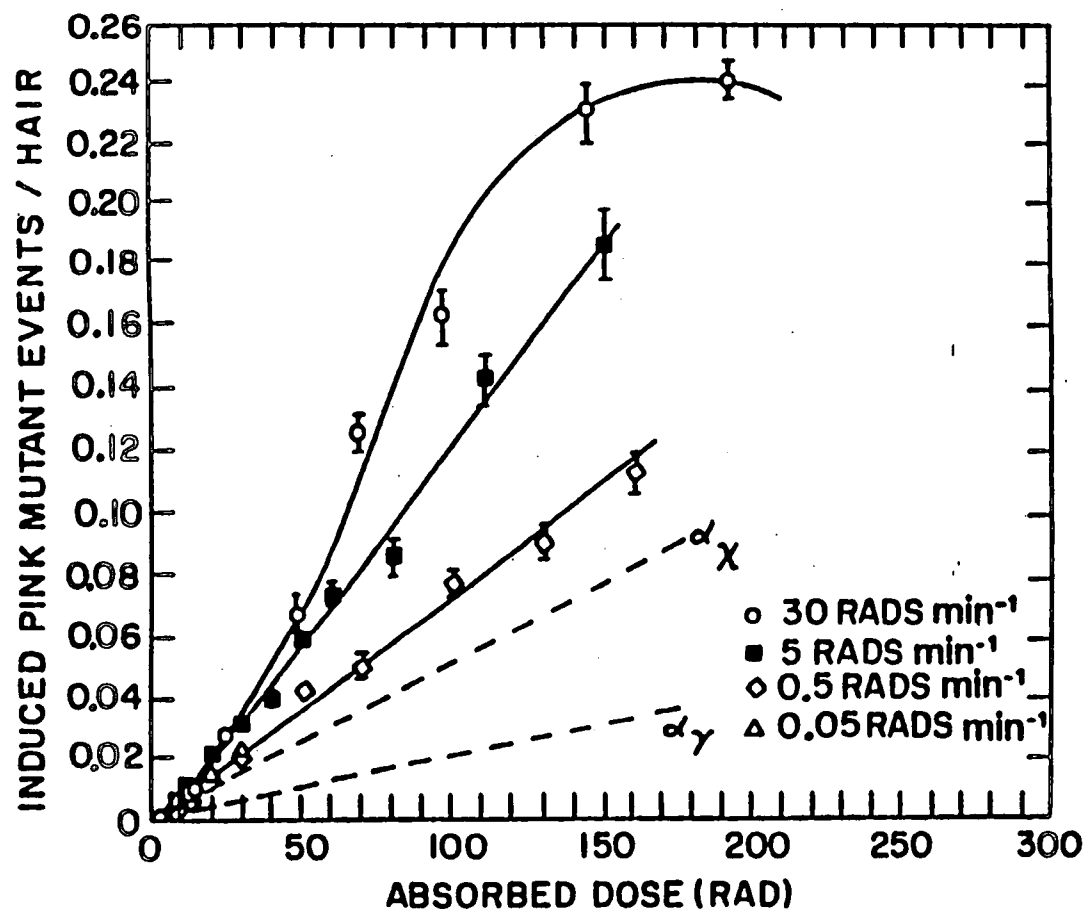


X-ray dose-response curve for induced pink mutations in *Tradescantia*, on a log log plot to show detail in the low-dose range.

NCRP 64

- 182
- 129 The same system demonstrates the effect of recovery and repair. As you increase from half a rad per minute up to 5 rads per minute, the slope of this linear portion of the curve changes quite dramatically.

# DOSE RATE EFFECTS



Dose-response curves for pink mutant events/hair after x irradiation at 0.05 and 0.5 rad min<sup>-1</sup> (combined in one line), and at 5 and 30 rad min<sup>-1</sup>.

129

131 Over the last three years, UNSCEAR has undertaken an in-depth study of hormesis. It tried to look at all of the scientific data that could shed some light on it, and prepared a summary report in 1994 which includes all of that material. However, their summary in the report to the General Assembly indicates that they don't think it's time to incorporate hormesis in our cancer risk projections. I think the important thing to remember here is that you can demonstrate hormetic or adaptive responses in some cell systems over some periods. Whether or not this has any place in terms of cancer or whether it has a place in which we haven't already experienced the hormetic effect, we're not yet certain. Remembering that we are living in a sea of background radiation, and if there is this hormetic effect, it could already be operating and be the reason for the health or lack of health of the population. Because there may be some adaptive response demonstrated in some phases of some cell lines, this doesn't give us enough information to take any leap of faith to think that that means that there is no effect at low doses in addition to doses we are receiving from natural background. That is an important thing to remember because whenever we talk about these effects, we are always talking about effects not in addition to a zero exposure, but in addition to an exposure above an already fairly high background of radiation.

## **UNSCEAR 1994 Report to the General Assembly**

### **Paragraph 33:**

**“Extensive data from animal experiments and limited human data provide no evidence to support the view that the adaptive response in cells decreases the incidence of late effects such as cancer induction in humans after low doses. However, further experimental studies should be conducted.”**

- 189
- 132 As you saw back in the *Tradescantia* data, there was a difference between doses delivered at a 0.5 rad/minute and those delivered at 30 rads/minute. We needed some way to judge this for the human being.

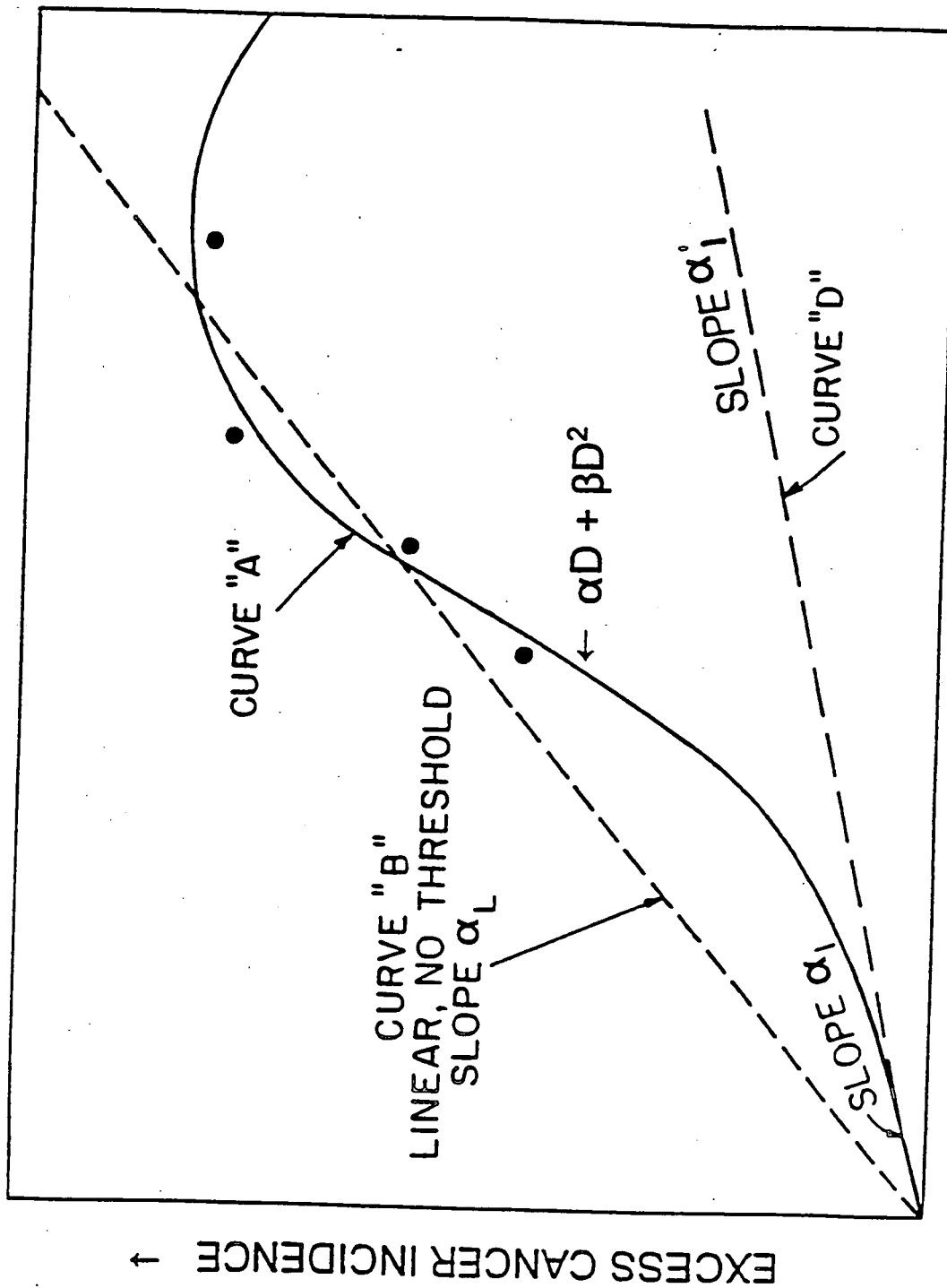
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# **DOSE / DOSE-RATE EFFECTIVENESS FACTORS**

133 I mentioned earlier one of the overheads on the Japanese data, that if I were to show you only one overhead, that would be it. I suppose if I had to pick two, this would be the second one. This is a figure which has been reproduced extensively since it was first presented in NCRP Report 64 in 1980. This was a report which was looking at the question of low doses and low dose rates. The question that is being asked is the one basically that I showed you with the *Tradescantia* data and in the early set of curves. Here in curve B is the linear dose if there was no effect of recovery and repair. This is the effect that you would get if there were recovery and repair and, in fact, the slope of this line suggests, as the *Tradescantia* data showed, that if you just kept increasing the dose, it would probably stay linear forever. On the other hand, if the dose rate goes up, then you will run into this region where there is an  $\alpha D + \beta D^2$ , a linear component, and a component that is due to multiple hits. The issue around the dose and dose rate has to do with how far below this slope we believe the true value will be. Again, I should point out that there is no question that trying to apply such data to the whole family of diseases we call cancer is going to lead us into some error. However, what we are trying to do is to get the best estimate that we can to establish the slope of  $\alpha_1$ .

# SCHEMATIC CURVES OF INCIDENCE VS. ABSORBED DOSE (NCRP, 1980)



ABSORBED DOSE →

- 139 We looked at uncertainties a little earlier. However, the NCRP has put together a report under the chairmanship of Warren Sinclair looking at uncertainties in the estimates of the probability of fatal cancer used in radiation protection. In that report, which gives the values you see in the overhead, they did assume (1) that there was a linear response at low doses, (2) that the lifespan study sample is representative of a U.S. sample. They did not account for any effect to the  $Q_{LET}$  effect for X rays and gamma rays, and (3) that the intestinal dose as a surrogate for the whole-body dose was acceptable. I think probably the most interesting part of this is the range of  $1.2 - 8.8 \times 10^{-2} \text{ Sv}^{-1}$  at the 90% confidence level. This analysis would suggest, given the assumptions, that  $5 \times 10^{-2} \text{ Sv}^{-1}$  or  $5 \times 10^{-4} \text{ rem}^{-1}$  is a reasonable number to use.

# UNCERTAINTIES IN THE ESTIMATION OF THE PROBABILITY OF FATAL CANCER

Nominal  $5 \times 10^{-2} \text{ Sv}^{-1}$

90% confidence level  
 $1.2 - 8.8 \times 10^{-2} \text{ Sv}^{-1}$

50th percentile  $3.38 \times 10^{-2} \text{ Sv}^{-1}$

- 158 We are going to examine the derivation of organ risk estimates to understand the range of uncertainties. We'll look at the relative organ contribution to the total risk. We'll look at population differences, age differences, and gender differences.

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158

## **ORGAN RISK ESTIMATES**

- Relative organ contribution to total risk
- Population differences
- Age differences
- Gender differences

159 The concept of total detriment drives the way in which the  $W_T$ s were calculated. The total detriment was taken to be the probability of fatal cancer which is certainly a detriment together with the relative length of life lost. That is, will the cancer under consideration result in a cancer which causes a loss of 30 years of life, or one which causes a loss of about 15 years of life? Then there is the relative non-fatal contribution, which is a way of incorporating in some way the idea that we shouldn't cast out from our considerations of detriment any cancer that doesn't kill you. On the other hand, we certainly don't want to equate a skin cancer with a fatal lung cancer.

## DERIVATION OF $W_T$ s

- Probability of fatal cancer/10,000/Sv
- Relative length of life lost  $1/\bar{l}$
- Relative nonfatal contribution

The product of these three contributions are used to obtain the relative contribution each organ makes to the total cancer detriment.

160 The lifetime mortality is derived not just from what we see in Japan, but materials that are taken from some of the medical case control studies, particularly in regard to breast, thyroid, and liver. The comparison is given for both ICRP (1977), which are the values used in NRC 10 CFR Part 20 revised and the one value used for ICRP Publication 60.

# LIFETIME MORTALITY IN A POPULATION OF ALL AGES FROM SPECIFIC FATAL CANCER AFTER EXPOSURE TO LOW DOSES

	Fatal probability coefficient ( $10^{-4} \text{ Sv}^{-1}$ )	
	ICRP (1977)	ICRP (1990)
Bladder	-	30
Bone marrow	20	50
Bone surface	5	5
Breast	25	20
Colon	0	85
Liver	-	15
Lung	20	85
Oesophagus	-	30
Ovary	-	10
Skin	-	2
Stomach	-	110
Thyroid	5	8
Remainder <sup>1</sup>	50	50
Total	125 <sup>2</sup>	500 <sup>3</sup>

<sup>1</sup>The composition of the remainder is quite different in two cases.

<sup>2</sup>This total was used for both workers and the general public.

<sup>3</sup>General public only. The total fatal cancer risk for a working population is taken to be  $400 \times 10^{-4} \text{ Sv}^{-1}$ .

- 161 The relative expected life lost was a new concept for ICRP which came about because the length of life lost is now one of the multi-attribute concepts needed to determine level of detriment we can find acceptable when we set our dose limits. Therefore, we needed to know how many years of life are lost from each of a number of cancers. Notice that there's not a great deal of disparity in those numbers with the exception of bone marrow, which is outstanding because you lose so much life from leukemia. The length-of-life factor is the length of life lost divided by the average over all of the cancer, which is given as 15 years. Note, for instance, first the leukemia with a loss of over 30 years, but for the bladder is only 9.8 years, meaning that it's a cancer of very old age which is true for the oesophagus and somewhat true for stomach cancer.

159  
10

**RELATIVE EXPECTED LIFE LOST PER FATAL CANCER IN DIFFERENT ORGANS,  
AVERAGED FOR TWO MODELS, SEX, AND FIVE NATIONAL POPULATIONS,  
AGE 0-90 Y, OR PER FATAL GENETIC EFFECT**

	Life lost (years) <i>l</i>	Factor <i>l/l</i>
Bladder	9.8	0.65
Bone marrow	30.9	2.06
Bone surface	15.0	1.00
Breast	18.2	1.21
Colon	12.5	0.83
Liver	15.0	1.00
Lung	13.5	0.90
Oesophagus	11.5	0.77
Ovary	16.8	1.12
Skin	15.0	1.00
Stomach	12.4	0.83
Thyroid	15.0	1.00
Remainder	13.7	0.91
Total	20.0	1.33

*l* is derived from the expected years of life lost for all cancers divided by the total number of fatal cancers, given as a group, and equals 15.0 years.

- 162 This is essentially the data we need if we're going to try to establish the criteria for non-fatal cancers based on the fatality fraction. The idea is that if one thinks about it, the fatality fraction is a measure of the viciousness of the cancer. Liver cancer at 0.95, with the 20-year lethality is 0.98. In a cancer like that, the existence of the cancer even though it isn't fatal is highly detrimental and the commission feels that that kind of cancer even if cured ought to be considered more highly detrimental than for example skin cancer which has a fatality fraction of 0.002. The 5-year data is given since it reflects the most recent information. However, it is likely to be an underestimate of the total fatality rate. The 20-year data is more complete, but does not adequately reflect the higher cure rates of today. The third column is a judgment based on both values.

# LETHALITY DATA FOR CANCERS IN ADULTS BY SITE (U.S. DHHS, 1989)<sup>1</sup>

	5 year 1980-85	20 year lethality 1950-70	Proposed lethality function $k$
Bladder	0.22	0.58	0.50
Bone	-	0.72	0.70
Brain	0.75	0.84	0.80
Breast	0.24	0.62	0.50
Cervix	0.33	0.50	0.45
Colon	0.45	0.62	0.55
Kidney	0.48	0.78	0.65
Leukemia (acute)	0.98	0.99	0.99
Liver	0.95	0.98	0.95
Lung and Bronchus	0.87	0.96	0.95
Oesophagus	0.92	0.97	0.95
Ovary	0.62	0.74	0.70
Pancreas	0.97	0.99	0.99
Prostrate	0.26	0.84	0.55
Skin	-	-	0.002
Stomach	0.85	0.90	0.90
Thyroid	0.06	0.15	0.10
Uterus	0.17	0.35	0.30

<sup>1</sup>Numbers were derived from tables and graphical data of U.S. by F.A. Mettler and W.K. Sinclair, ICRP Publication 60.

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- 163 This gives you the calculation of the way in which the fatality, the non-fatal component, was used. F is any number of fatal cancers in the listed organs. This calculation says you should multiply the fatal cancer coefficient by 2-k. Remember that this is obtained in this formulation by multiplying the total of non-fatal cancers by the fatality fraction.

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## NON-FATAL COMPONENT

Given  $F$  = fatal cancers  
 $k$  = fatality fraction

$\therefore$  Total cancers =  $F/k$

and non-fatal cancers =  $(1-k)F/k$

Non-fatal cancer x the fatality fraction =  
 $k(1-k)F/k = (2-k)$

$(2-k)$  is then multiplied by the fatal cancer coefficient

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- 164 This includes the overall approach the Commission took in establishing the relative contribution of each of the organs to the total detriment. We multiply the probability of fatal cancer times the relative length of life lost times the relative non-fatal contribution and end up with a product. We then establish the fraction of the total detriment which came from that organ. If you notice in our example, we end up with a product of 80.3 for lung cancer which is 11% of the total detriment. For skin cancer the product is 4 and it represents 0.6% of the total detriment.

## RELATIVE CONTRIBUTION OF ORGANS TO THE TOTAL DETRIMENT

Probability of fatal cancer (per 10,000 per Sievert)	Relative length of life lost <i>ll</i>	Relative non-fatal contribution	Product
<b>Example: Lung Cancer</b>			
85	.9	1.05	80.3 (11%)
<b>Example: Skin Cancer</b>			
2	1	2	4 (.6%)

- 165 This is the final output of all of that activity and the relative contribution you see on the right takes all of these products and places them in their relative role with reference to the percent of the total detriment. You can see that these are carried to three significant figures and that there is a great deal of variation. As a matter of fact, there are no two numbers alike in that relative contribution table.

## RELATIVE CONTRIBUTION OF ORGANS TO THE TOTAL DETRIMENT

Organ	Probability of fatal cancer per 10,000 per Sievert	Relative length of life lost	Relative non- fatal contribution	Product	Relative Contribution
Bladder	30	0.65	1.50	29.4	0.040
Bone marrow	50	2.06	1.01	104.0	0.143
Bone surface	5	1.00	1.30	6.5	0.009
Breast	20	1.21	1.50	33.4	0.050
Colon	85	0.83	1.45	102.7	0.141
Liver	15	1.00	1.05	15.8	0.022
Lung	85	0.90	1.05	80.3	0.111
Oesophagus	30	0.77	1.05	24.2	0.034
Ovary	10	1.12	1.30	14.6	0.020
Skin	2	1.00	2.00	4.0	0.006
Stomach	110	0.83	1.10	100.0	0.139
Thyroid	8	1.00	1.90	15.2	0.021
Remainder	50	0.91	1.29	58.9	0.081
Gonads		1.33	-	133.3	0.183
Total	500			725.3	1.000

166 These are the tissue weighting factors. Now, remembering that we were going to use those relative contributions to get us to our tissue weighting factors, the question quite obviously arises, how did we arrive at these numbers: 0.01, 0.05, 0.12, 0.20? We talked long and hard about whether this should be 1, 5, and 10 or 1, 3, and 9, but the important thing to notice is that we have, with the exception of the gonads, three categories of weighting. We've got those organs which are at high risk, the colon, lung, stomach, and bone marrow. We have those that are at moderate risk, the breast, liver, oesophagus, thyroid, and the remainder tissues, and we have the skin and bone which are relatively insensitive. The impression you should get from this table is that we can't justify the precision that was implied in the previous table. One of our objectives is to demonstrate this lack of precision. You will notice, however, that 0.12 is given for one set of weights. The reason for that was that if we didn't have enough data to be precise about our figures, why should we change the previous weight for the bone marrow and lung which had been at 0.12? We did, however, keep all of the "high" risk tissues at 0.12.

## TISSUE WEIGHTING FACTORS

Tissue or Organ	Tissue Weighting Factor, $W_T$
Skin	0.01
Bone surface	0.01
Bladder	0.05
Breast	0.05
Liver	0.05
Oesophagus	0.05
Thyroid	0.05
Remainder	0.05
Bone marrow (red)	0.12
Colon	0.12
Lung	0.12
Stomach	0.12
Gonads	0.20

- 167 This is a demonstration of the variability of the relative probability of cancer in organs across various populations. This was important because eventually what we did was to average across these populations, across ages, and across gender. My overall objective in these next few overheads is to indicate the variation in all these parameters which points out that a given value of  $W_T$  should never be used for an individual measure of risk. One of the variables is a function of where you live. In particular, look at the difference in the incidence of cancer of the esophagus between the United States and Puerto Rico. The incidence of stomach cancer is different between Japan and the United States, but the United States is also very different from Puerto Rico, as is China and the United Kingdom. There is great variation among all of these organs and it is just one of the uncertainties that I want you to be aware of.

**RELATIVE PROBABILITIES OF FATAL CANCER IN ORGANS  
VS. POPULATION TYPE, MALE AND FEMALE, AGE 0-90,  
MULTIPLICATIVE MODEL**

Organ	Japan	United States	Puerto Rico	United Kingdom	China
Esophagus	0.038	0.014	0.098	0.03	0.269
Stomach	0.291	0.033	0.136	0.05	0.224
Colon	0.18	0.32	0.206	0.225	0.103
Lung	0.174	0.205	0.141	0.274	0.097
Breast	0.023	0.075	0.048	0.085	0.022
Ovary	0.014	0.031	0.016	0.031	0.019
Bladder	0.052	0.076	0.078	0.09	0.036
Bone Marrow	0.077	0.096	0.127	0.064	0.079
Remainder	0.15	0.15	0.15	0.15	0.15
All Cancer	0.999	1	1	0.999	0.999

- 168 This is a demonstration of the variation of a fatal cancer incident as a function of age. Look particularly at the 0-19 y column and the 20-64 y column and you'll see that there is a strong variation of the esophagus (0.021 for the 0-19 y and 0.061 for the 20-64 y). The risk of the bladder cancer for 0-19 y, 0.030, and for the adults, 0.082. Even the bone marrow is doubled for the adult.

**RELATIVE PROBABILITIES OF FATAL CANCER IN ORGANS  
VS. AGE GROUP (0-90 Y, 0-19 Y, 20-64 Y) JAPANESE POPULATION,  
AVERAGE OF MALE AND FEMALE**

Organ	0-90 y	0-19 y	20-64 y
Oesophagus	0.038	0.021	0.061
Stomach	0.291	0.266	0.305
Colon	0.180	0.255	0.089
Lung	0.174	0.191	0.159
Breast	0.023	0.025	0.022
Ovary	0.014	0.009	0.023
Bladder	0.052	0.030	0.082
Bone marrow	0.077	0.052	0.109
Remainder	0.150	0.150	0.150
All cancer	0.999	1.000	1.000

- 169 Here we examine how the risk estimates are affected by the difference in gender. For example, for the multiplicative model, incidence of colon cancer in men is almost twice as high as in women. The bone marrow (leukemia) is significantly higher in men than in women.

Q.E.D. Don't use  $W_T$  for individual risk estimation.

**RELATIVE PROBABILITIES OF FATAL CANCER IN ORGANS VS. SEX  
AND PROJECTION MODEL (JAPANESE POPULATION, AGE 0-90 Y)**

Organ	Projection Model			
	Additive		Multiplicative	
	M	F	M	F
Oesophagus	0.039	0.065	0.031	0.044
Stomach	0.225	0.223	0.319	0.262
Colon	0.067	0.066	0.127	0.232
Lung	0.118	0.160	0.184	0.164
Breast	---	0.076	---	0.046
Ovary	---	0.065	---	0.029
Bladder	0.092	0.034	0.081	0.024
Bone marrow	0.307	0.158	0.106	0.040
Remainder	0.150	0.150	0.150	0.150
All cancer	1.000	1.000	1.000	1.000

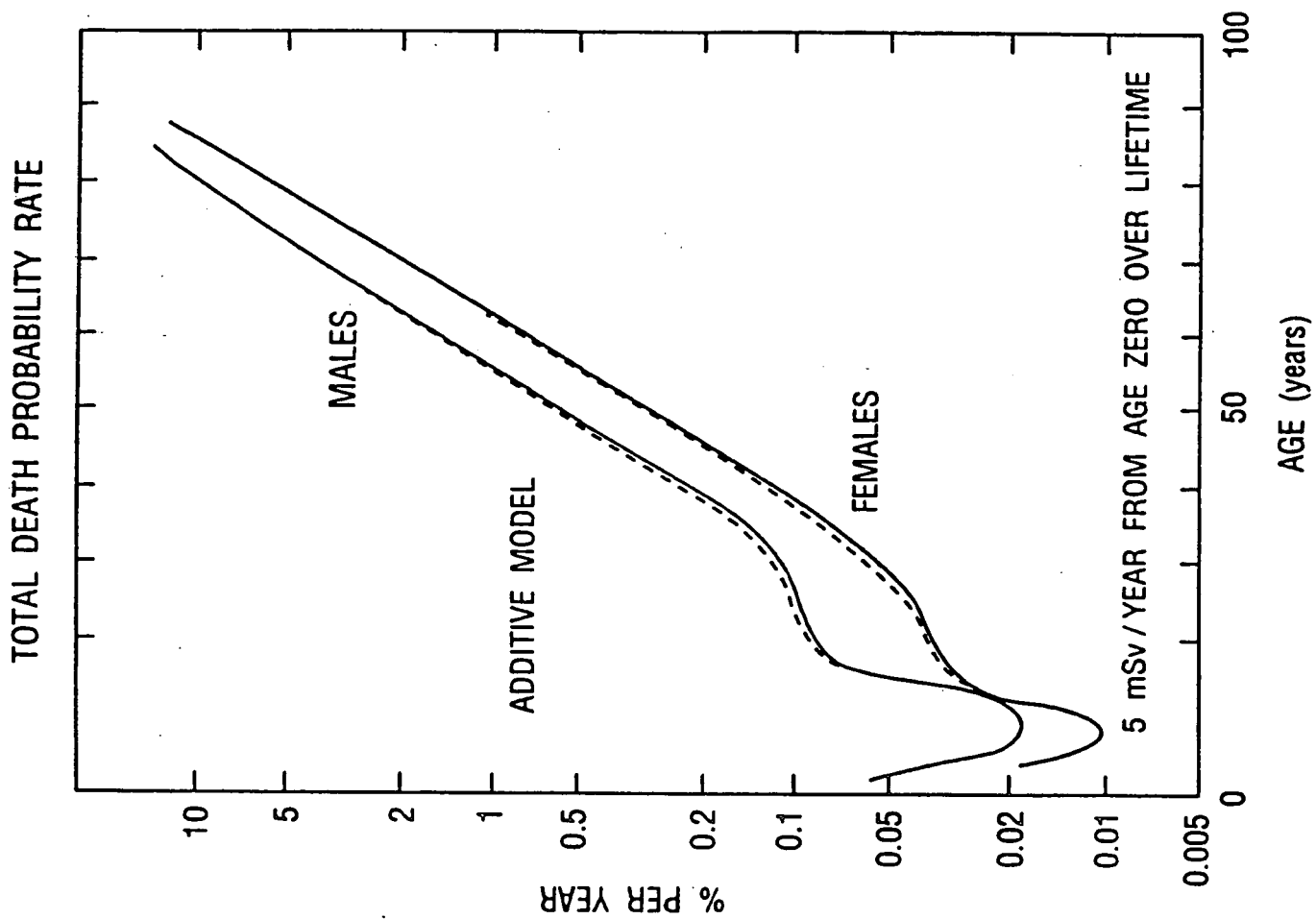
- 189 The attributes of the exposure of ages 0 to 75 looks a lot like the data that I showed you for the workers but, in fact, the numbers are a factor of ten smaller. It does indicate lifetime level of aggregate detriment is about .6% at 1 mSv per year.

## ATTRIBUTES OF DETRIMENT DUE TO EXPOSURE AGE 0-75

Annual effective dose (mSv)	1	2	3	5
Probability of attributable death (%)	0.4	0.8	1.2	1.99
Weighted contribution from non-fatal cancer (%)	0.08	0.16	0.24	0.40
Weighted contribution from hereditary effects (%)	0.1	0.21	1.31	0.52
Aggregated detriment (%)	0.6	1.2	1.8	3
Time lost due to an attributable death given that it occurs (y)	13	13	13	13
Mean loss of life expectancy at age 0 years (y)	0.05	0.11	0.16	0.27

190 I think this is a very interesting plot. It is the death probability rate; the rate at which people die as a function of age. You'll notice first that males and females are quite disparate in the way in which this happens. Notice, too, that the females are much lower. At the very young ages, people are at some risk, 0.05% per year, but as they get to be about 3 years old, the risk really goes down very, very low and reaches a minimum for females which is 0.01% per year. Shortly thereafter, both the males and particularly the males go way up by the time they are in their early teens -- when the hormones are racing through the body and the automobiles are racing through the streets. We see that there is a big bump in the death probability rate in those middle teen years. Then the rate increases, pretty much on a straight line up to ages determined by the lifespan of the population. What's interesting about this kind of data is that if we were to look 20 years ago, we might find the whole set of curves shifted to the left. The females might take the place of the males and the males would be further left or even twice as far. This really is a function of how safe society is. The dashed curves you see are the way the curves would look if everybody in the population received 5 mSv per year from age 0 over their lifetime, that is 500 mrem. You would have a new curve which would be the shape of that dotted curve, which, as you can see, doesn't make a big change in the death probability rate.

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- 191 Having considered all of this, the ICRP 60 recommendation for the public is that it should be no more than 1 mSv per year averaged over any five consecutive years and did reduce the annual dose equivalent for the lens, the skin and the hands are 15 and 50 mSv respectively. I think it is interesting to note that the ICRP had changed its recommendations given in Publication 26, that is a recommendation of 500 mrem per year in its Paris statement of 1985. At that time the commission said that its present view is that the principle limit is 1 mSv in a year but it is permissible to use a subsidiary dose limit of 5mSv in a year for some years provided that the average annual effective dose equaled over a lifetime does not exceed the principle limit of 1 mSv in a year. This is not a new idea for Publication 60. It had been on the books since 1985.

# ICRP PUBLICATION 60 RECOMMENDED DOSE LIMITS FOR THE PUBLIC

Effective Dose	1 mSv per year averaged over any 5 consecutive years
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Annual equivalent dose in	
the lens of the eye	15 mSv
the skin (100 cm <sup>2</sup> )	50 mSv
the hands	50 mSv

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Here, it's the NCRP recommendation for the public, 1 mSv (100 mrem) per year allows 5 mSv per year for infrequent exposure. We found that this isn't very helpful, actually, and we are probably going to try to modify our recommendations so that they're more in accordance with those of ICRP which allows the averaging rather than this infrequent exposure.

# **NCRP REPORT 116 DOSE LIMITS FOR THE PUBLIC**

**1 mSv (100 mrem) per year  
allows 5 mSv/yr for infrequent exposure**

193 NCRP source-related limits for members of the public. The important issue here is one of apportionment. If you're going to have a number of sources which are going to expose the members of the public to some limit, then you've got to apportion that limit so that the individual member of the public over whom no one has any control will be still within the 100 mrem limit. The NCRP suggested that about a quarter of the dose limit (25 mrem). Although the NCRP did suggest that if the affected individual who needed more than the 25 mrem wanted to establish that there were no other known sources which could be irradiating the public, that he might be able to justify a higher number. I might note that the CEC is using about 0.3 mSv per year much along the same line.

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## **NCRP SOURCE-RELATED LIMITS FOR MEMBERS OF THE PUBLIC**

25 mrem (.25 mSv) per year  
unless all other sources are known

CEC - .3 mSv/yr

194 It is interesting that the NRC has adopted public dose limits of the ICRP and NCRP recommendations.

## NRC PUBLIC DOSE LIMITS

1 mSv (100 mrem)/yr may allow 5 mSv  
(500 mrem)/yr based on demonstrated need

Internal exposure limits based on .5 mSv  
(50 mrem/yr)

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195 One of the topics that I was asked to review in the course was about ALARA and the dollars per person-rem. It should be very clear that the system of protection for ICRP, NCRP, and NRC is that ALARA is simply the most important controlling criterion. Dose limits are unsatisfactory for that purpose and it is ALARA that we have to really bear down on.

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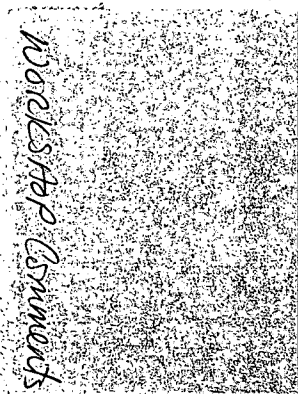
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February 17, 1999

Ms. Carla Sanda  
Ms. Anna Corbett  
AIMSI  
5460 Ward Road #370  
Arvada, CO 80002


Dear Carla and Anna:

As followup to the February 11 workshop on risk provided for the RSALOP by Dr. Charles Meinhold I am sending the following two items:

- 1) A letter with attachments addressed to Dr. Meinhold raising questions about his presentation;
- 2) A paper entitled "Limitations of the ICRP Recommendations for Worker and Public Protection from Ionizing Radiation" by Canadian radiation specialist Dr. Rosalie Bertell (originally prepared for the European Parliament).

Please make these available to members of the RSALOP.

Thank you,

  
LeRoy Moore

# Rocky Mountain Peace and Justice Center

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February 16, 1999

Dr. Charles B. Meinhold, President  
National Council on Radiation Protection and Measurements  
7910 Woodmont Avenue, Suite 800  
Bethesda, MD 20814

Dear Dr. Meinhold:

Thank you for the very informative workshop on radiation health risk you gave in Broomfield, Colorado, on February 11 for the Rocky Flats Radionuclide Soil Action Levels (RSALs) Oversight Panel. During your presentation I raised several questions which I want by means of this letter to pursue further, since we had entirely too little time during the workshop to discuss them adequately.

1) My first line of questioning concerns the relative biological effectiveness (RBE) of alpha emitters, such as plutonium. The RBE specifies how damaging a dose received internally from a given alpha emitter may be by comparison to a dose of the same magnitude received externally from gamma radiation. Typically, internal alpha emitters are much more damaging. Specifying the appropriate RBE is crucial for calculating risk. An incautious calculation can greatly underestimate potential harm. The National Council on Radiation Protection and Measurements (NCRP) and the International Commission on Radiological Protection (ICRP) recommend using an RBE of 20 in calculating risk from plutonium exposure (ICRP Publication 26, 1977; and ICRP Publication 60, 1990). This number was used by DOE, EPA, and the Colorado Department of Public Health and Environment in setting the RSALs for Rocky Flats. The agencies believe that in following the lead of NCRP and ICRP they are on firm footing, but I question whether these bodies themselves are on firm footing in terms of evidence.

As you explained, any given radionuclide has a range of RBEs, depending on the end-points in terms of disease or disability. Thus, one of your overhead projections showed a table (I did not get the reference) that gave a range of RBEs for alpha-emitters of from 1 to 100. That the high end of the scale was only 100 surprised me, since as early as 1979 R. J. DuFrain et al of Oak Ridge in *Health Physics*, 37: 279-289, calculated that the appropriate RBE for alpha-induced cytogenetic (hereditary) damage was 278 -- almost triple the upper end on the table you showed. Moreover, an August 1997 Draft Report called "Assessing Risks of Exposure from Plutonium" (RAC Report No. 5 CDPHE-RFP-1997-Draft), written by Helen A. Grogan, Warren K. Sinclair, and Paul G. Voilleque as part of the Rocky Flats Dose Reconstruction Study, provides much detail on this topic. From their survey of a large body of research they report RBE's for plutonium ranging as high as 110 for lung cancer, 350 for bone sarcomas, 360 for hematopoiesis -- that is, from 5.5 to 12 times as high as ICRP's 20. Regarding ICRP and NCRP they comment: "Differences in RBE between different biological tissues or for plutonium as opposed to any other alpha-emitter have not been considered in detail by these organizations" (p. 6-30). ICRP's 1990 Publication 60 makes the very same RBE proposal as ICRP's 1977 Publication 26. It appears that ICRP and NCRP aren't doing their homework.

Staying, however, with the scale you presented, if 100 RBE is the upper end of the scale, why do ICRP and NCRP recommend 20 as the appropriate RBE for all alpha exposures? Does this result from taking an averaging approach, as if harm resulted from average exposure distributed throughout the body rather than from a discrete exposure to a distinct organ for which there would be a specific RBE. If ICRP and NCRP have to recommend a single RBE number for the political expedience of setting radiation standards, wouldn't it be more in keeping with the task of protecting public health to take a mean approach rather than an averaging one? Staying with your numbers, this would result in a recommended RBE for alpha-emitters not of 20 but of 50. There would still be numerous cancers for which the upper range RBE would be above 50. Of course, a most-cautious approach would employ the highest RBE, a more cautious approach at least a number well above the mean. Getting a more realistic number matters. What is at stake is the health of certain people in the population, including people who live near or work at Rocky Flats.

Further to the point of RBE, Eric G. Wright and his colleagues at the Medical Research Council at Harwell, Oxfordshire, conclude from their research that the RBE for alpha-induced chromosomal damage is "effectively infinite" (see their letter to *Nature*, vol. 355, 20 Feb. 1992, pp. 738-740 [enclosed]). An article by Rob Edwards in *New Scientist*, October 11, 1997, pp. 37-40 (also enclosed), discusses the research of Wright and others focused on what Wright calls "radiation-induced genomic instability" -- that is, chromosomal damage that could permanently pollute the human gene pool. Such instability, Wright and his associates say, can result from a dose as low as 0.5 grays of alpha radiation -- "the equivalent of a single alpha particle passing through a cell, the lowest dose the cell could receive." Edwards quotes Jack Little of the Harvard School of Public Health: "Genomic instability changes our way of thinking about how radiation damages cells and produces mutations." Further, according to a report from a 1995 World Health Organization (WHO) conference in Helsinki, genomic instability is also a "plausible mechanism" for explaining illnesses other than cancer. Such illnesses may prove so elusive that epidemiology is "powerless" to detect any relationship between their incidence and exposure to radiation. In the light of findings like these, Edwards, in a remarkable understatement, says, "the regulatory system starts to look inadequate."

From the preceding paragraph I draw two questions: How does NCRP respond to the work of Wright and others on the whole question of radiation-induced genomic instability and its implications for setting standards for permissible exposure to radiation? The response needs to consider the whole body of studies mentioned in Edwards' article, including the unpublished report of the 1995 WHO Helsinki conference on this topic. Second, what about the possibility that radiation-induced genomic instability may contribute to illnesses other than cancer? How is NCRP incorporating this issue into its work?

2) During your presentation you emphasized that NCRP's recommendations regarding risk of exposure to radiation are based on a linear/no threshold approach. NCRP thus assumes that there is no such thing as a safe dose above natural background level, and that harm is commensurate with the dose. I asked at the time why NCRP didn't employ the more cautious supralinear approach proposed by researchers who say they find heightened damage to the organism at very low levels of exposure. In response you said that some quite unhealthy individuals are biologically susceptible to disease from low-

dose exposure, and that adjusting standards for general exposure to these unhealthy persons would mean they might die at age ten rather than eleven. With this answer you really missed the point of my question, for I was asking about the possible harmful effects of very low-dose exposure to otherwise healthy persons within the population in general.

One of the researchers who advocates a supralinear approach is Karl Z. Morgan, long-time head of health physics at DOE's Oak Ridge facility and your predecessor within both NCRP and ICRP. Enclosed is a copy of an interview with Morgan in which he explains why he has concluded that a supralinear approach best fits the data. He says that "down at the very low doses you actually get more cancers per person-rem than you do at the high doses." He doesn't mean that more cancers result from low-dose exposure but that "damage per unit dose is greater at these levels." This is true "in part because the high levels will more often kill cells outright, whereas low levels of exposure tend to injure cells rather than kill them, and it is the surviving, injured cells that are the cause for concern." I'm curious how NCRP responds not simply to these words of Morgan's but to the published papers in which he and others have provided the basis for adopting a supralinear by contrast to a linear approach in calculating risk from very low-dose exposure.

3) This brings me to my final line of questioning. In your talk you said that between 1977 and 1990 NCRP and ICRP upped their understanding of the risk posed by exposure to radiation fivefold. That is, by 1990 these two influential bodies saw radiation as five times more dangerous than they had thought only thirteen years earlier. Radiation hadn't become more dangerous. It was only seen to be. (Oddly, this did not affect ICRP/NCRP recommendations regarding the RBE for alpha emitters.) I asked whether we can expect a similar change in assessment of risk over the next fifteen to twenty years. When you in effect said, "No, because our understanding has more or less stabilized," I felt like I was in church listening to a preacher whose words weren't quite believable.

It seems to me that, while we have learned a great deal about potential harm from radiation exposure, our understanding is still very incomplete. You referred in your own talk to myriad uncertainties. An article by Rudi H. Nussbaum and Wolfgang Köhnlein in *Environmental Health Perspectives*, vol. 102, No. 8 (August 1994), 656-667, examines a large body of recent research on negative health effects from low-dose exposure that challenge prevailing understandings. The studies they review need to be carefully appraised. In addition, I invite you to consider: What if ICRP and NCRP greatly underestimate the appropriate RBE for alpha emitters? And what if those concerned about radiation-induced genomic instability are correct? Or what if a supralinear approach is not simply safer but also more appropriate for determining risk? Then there's Alice Stewart, the person who four decades ago identified harm to fetuses in utero from x-rays of the mother. She says standards for permissible exposure should be based not on data from Japanese survivors of one-time high-dose events but on exposure to very low doses that may or may not be long-term and continuous. There seems ample reason to suppose that in the near future radiation may be recognized as more dangerous than admitted today by NCRP or ICRP. This is not an idle concern for people who live near a facility like Rocky Flats, or for those of us charged with the task of overseeing a review of the Rocky Flats Radionuclide Soil Action Levels.

I put the foregoing issues before you because the answers you provided to my questions at the time of your workshop were inadequate and not convincing. I realize my questions are not small ones and that answering them will be complicated. If you yourself or others within NCRP can provide answers I will be grateful. I commend you not only on your knowledge of a very complex subject but also on your ability to make it clear to others. Those of us who are concerned about low-dose radiation emissions from a facility like Rocky Flats need to hear from people like you, just as you need to hear from us. Such exchange will lead to science which will be both better in a technical sense and more credible to the affected public.

The BEIR VII study just now being undertaken under the auspices of the National Academy of Sciences will examine the current state of knowledge regarding low-dose exposure with a view to possibly recommending changes in standards for permissible exposure. I therefore am forwarding this letter to the director of the BEIR VII committee, since the issues raised here need to be part of their homework.

In the belief that you could influence the way the BEIR VII work will be carried out, I urge you to insist that this study be done in the most open way possible, from literature search to determination of scope of the study to hearing and responding to concerns of affected populations around nuclear facilities across the country to presenting findings and conclusions in ways that are convincing and thus acceptable to the public. Science of this sort is too important to be conducted behind closed doors. On behalf of your fellow citizens, please weigh in on the side of openness and public participation in every aspect of the BEIR VII study. Lacking this will only perpetuate the current distrust of nuclear science and the nuclear industry -- and, of course, of the government agencies which implement recommendations from bodies like NCRP, ICRP, and the BEIR VII committee.

I will welcome an early response to the questions posed here.

Yours sincerely,

*deRoy Moore*  
LeRoy Moore, Ph.D.

Enclosures:

M. L. Kadhim et al, article from *Nature*  
Rob Edwards article from *New Scientist*  
Robert Del Tredici interview with Karl Z. Morgan

cc: ✓ Rocky Flats Radionuclide Soil Action Levels Oversight Panel  
John Till, Risk Assessment Corporation  
Rocky Flats Citizens Advisory Board  
BEIR VII Committee, c/o Rick Jostes, Study Director, BEIR VII, National Research Council

# **Limitations of the ICRP Recommendations for Worker and Public Protection from Ionizing Radiation**

For Presentation at the STOA Workshop  
Survey and Evaluation of Criticism of Basic Safety Standards  
for the Protections of Workers and the Public against Ionizing Radiation  
European Parliament, Brussels, 5 February 1998

## **ABSTRACT:**

The mathematical and biological elegance of the International Commission on Radiological Protection (ICRP) intellectual structure, which has the obvious mark of the physicist, should not be allowed to blind us to its inability to address the full spectrum of worker and public health problems caused by the routine and/or accidental exposures to ionizing radiation inseparable from the operations in the nuclear fuel cycle. I am referring to the very narrow administrative decisions which limit the focus of ICRP concern, and make possible the simplifications designed for administrating its recommendations. For example, the recognized biological endpoints deemed to be of concern for regulatory purposes are limited to: radiation induced fatal cancers and serious genetic diseases in live born offspring.

There are many administrative decisions embedded into the elaborate (artificial) methodology for calculating effective whole body dose and for calculating the expected number of radiation induced fatal cancers. The strengths of the ICRP approach rest primarily on its ability to quickly convert a multidimensional problem, that is, a mixture of radionuclides, having a variety of energies and types of emissions, multiple pathways to humans, and a variety of target human organs, into a linear system amenable to management decisions. This is a recognized mathematical achievement. However, in risk assessments, long term chronic exposure, the aftermath of a disaster, or in worker compensation hearings, these same techniques cloud reality and work effectively against justice for the victims. The elegant mathematics must not be allowed to cover up the injustices.

In terms of its own claims, ICRP does not offer recommendations of exposure limits based on worker and public health criteria. Rather, it offers its own risk/benefit trade off suggestion, containing value judgements with respect to the "acceptability" of risk estimates, and decisions as to what is "acceptable" to the individual and to society, for what it sees as the "benefits" of the activities. Since the thirteen members of the Main Committee of ICRP, the decision makers, are either users of ionizing radiation in their employment, or are government regulators, primarily from countries with nuclear weapon programs, the vested interests are clear. In the entire history of the radiologist association formed in 1928, and ICRP, formed when the physicists were added in 1952, this organization has never taken a public stand on behalf of the public health. It never even protested atmospheric nuclear weapon testing, the deliberate exposure of atomic soldiers, the lack of ventilation in uranium mines, or unnecessary uses of medical X-ray.

This paper will examine the credibility of the Atomic Bomb Studies as a basis for the radiation protection standards, the adequacy of the biological mechanisms and endpoints chosen for standard

setting, the adequacy of research on other possible biological mechanisms and endpoints, and the decisions made by ICRP on the "acceptability of the detriment" to the individual and to society, relative to comparable decisions made by health professionals for chemical hazards.

### THE ATOMIC BOMB STUDIES:

The atomic bomb studies followed, and did not precede the setting of the radiation protection guidelines recommended by ICRP and followed internationally until 1990. The main recommendations were set in 1952, and the first doses assigned to A-bomb survivors were not available until 1965. Moreover, the research was designed to determine the effects of an atomic bomb, not the health effects of exposure to ionizing radiation. The research was undertaken by military researchers from both the US and Japan familiar with and primarily concerned with military use of atomic, chemical and biological warfare agents. The research has come too late for standard setting needs, it has focused on cancer deaths, is uncorrected for healthy survivor effect, and is not inclusive of all of the radiation exposures of cases and controls (dose calculations omit fallout, residual ground radiation, contamination of the food and water, and individual medical X-ray), and fails to include all relevant biological mechanisms and endpoints of concern.

It is normally claimed that biological basis of the cancer death risk estimates used by ICRP, is the atomic bomb studies. However, these studies are not studies of radiation health effects, but of the effects of an atomic bomb. For example, the radiation dose received by the Hiroshima and Nagasaki survivors from fallout, contamination of food, water and air, has never even been calculated. Only the initial bomb blast, modified by personal shielding, is included in the US Oak Ridge National Laboratory assigned "dose". This methodology is carried to an extreme. For example, one survivor I know lived within the three kilometer radius of the hypocenter, but was just beyond the three kilometre zone, at work, when the bomb dropped. As soon as she could, she returned home after the bombing and found her parents and brother dead. Then she stayed in her family home for the three following days, not knowing where to go and filled with grief. Although she suffered radiation sickness and many subsequent forms of ill health, she is counted as an "unexposed control" in the atomic bomb data base. By using the "not in the city" population which entered after the bombing as "controls", many of cancers attributable to the radiation exposure in both cases and controls are eliminated from the outcomes considered related to the bomb. In contrast, in the United States:

"Any veteran exposed to a nuclear bomb test or who was part of the first 11 months of occupation of Hiroshima or Nagasaki is provided coverage for radiation exposure and any such veteran is assured priority of hospital treatment ahead of veterans with non-survivor claims. Occupation of Hiroshima or Nagasaki means official military duties within ten miles of either city, between the dates of 6 August 1945 and 1 July 1946." (Ref. 1)

The difference is obvious: the A-bomb studies measure only cancers due to the bomb blast; veterans are compensated for radiation induced cancers.

The basic radiation protection standards, recommended by ICRP and in effect until 1990, were set

by the physicists of the Manhattan Project and presented to the International Association of Radiologists in 1952, when they asked to be allowed to join the organization. They set maximum permissible doses per year as 50 mSv for workers and 5 mSv for the public.

The data base for the Hiroshima and Nagasaki Life Span Study, the basis for the mortality estimates, was first identified in the 1950 Japanese Census. The information was not collected and ready for analysis until around 1957, and because it depends on first cause of death information, it was based on only a small percentage of deaths for the first seven years. It was heavily dependent on the accuracy of death certificates. Deaths in the Hiroshima and Nagasaki population between 1945 and 1950 are not included in the study. Even today, the majority of the 1950 identified survivors are still alive. (Ref. 2)

The first research reports were based on distance from the hypocentre. The doses were not assigned to the survivors until the T65D, (which stand for tentative dose estimates, 1965), compiled by John Auxier of Oak Ridge National Laboratory, became available. Atomic Bomb dose/response studies could not have been the basis of recommendations set in 1952 because they did not exist!

Interestingly, the Atomic Bomb Casualty Commission (ABCC) and its successor organization, the Radiation Effects Research Foundation (RERF), has since the beginning collaborated with the Japanese National Institute of Health (JNIH). ABCC was set up by the occupying force in September 1945. Their Japanese partner was responsible for hiring and firing all Japanese scientists who worked on the A-bomb data, although the US assumed singular control of all of the dose assignments once they were available. The JNIH was actually established by the order of the U.S. Forces (Ref. 3), staffed with scientists from the Institute of Infectious Disease (IID) attached to the University of Tokyo, and containing most of the leading medical scientists from the Japanese Biological Warfare (BW) Institutions and the infamous Unit 731, which was responsible for the gross experimentations with humans in Manchuria during World War II. (Ref. 4) The Japanese scientists who engaged in biological warfare experiments on live human beings, allegedly including allied prisoners of war, were granted immunity by the U.S. Army from investigation for war crimes in return for the results of their experiments. Kobayashi Rokuzo, advisor to the IID laboratory was attached to the Japanese Army's Medical College headquarters of the BW network, was Director of JNIH from 5/47 to 3/55. His Vice-Director for the same term was Kojima Saburo, who had intensively cooperated with BW Unit 1644 in the vivisection of humans at Nanking, and with the IID unit during the occupation of China. The Director of the JNIH from 3/55 to 4/58 was Komiya Yoshitaka, who was a member of the Institute of Health in Central China during the occupation, part of the BW network of hospitals run by the Military Police. Yanagisawa Ken, Vice-Director from 10/58 to 3/70, conducted experiments on Chinese youths during the occupation, through BW Unit 731. It was through these human experiments that he developed dried BCG, becoming "eminent" in medical circles. The list is much longer, including Directors and Vice-Directors up until 1990, scientists known to have conducted military experiments on humans. (Ref. 5).

Clearly warfare and the results of the nuclear bomb "experiment" were the main guiding principles of the research at Hiroshima and Nagasaki. American researchers were "safe" with the Japanese who

had also conducted research on humans in order to further their war tactics. Consequently, it was not until 1994 that the research on cancer incidence rate after the A-bomb exposure was first published, highlighting their neglect the high incidence rate of breast, thyroid and skin cancers (not always fatal). Incidence rate had been unreported up until then (Ref. 6).

In 1986, we witnessed the release of a complete reassignment of doses to the Hiroshima and Nagasaki survivors, supposedly based both on revised estimates of the neutron component of the dose and new estimates of shielding. According to Dr. Dale Preston, who directed the reassignment of doses, this was not a simple proportional change in all doses, but a true reassignment, often to new categories of exposure. This implies that all of the research based on the earlier assignment of doses is now considered to be wrong.

"The importance of the new research is that it completely changes the scheme of radiation doses that people are supposed to have received in Japan, particularly in Hiroshima." (Ref. 7)

According to this same article, the dispute over dose estimates had been brewing for four years, since 1977, when the US National Council on Radiation Protection asked John Auxier for supporting information for his assignment of doses to atomic bomb survivors. Auxier stated that when his office was moved in 1972, the record division at Oak Ridge mistakenly shipped his files to the shredder. He never reported the loss of these valuable papers. There was no US Government response until 1981 and it took until 1990 to complete this rearrangement of the Hiroshima and Nagasaki data. All of this manipulation of data took place "in house" by the staff of the US Department of Energy. Such sweeping change in a data base is usually considered manipulation, whether deliberate or not.

There are other reasons to challenge the ICRP reported reliance on the atomic bomb studies for its fatal cancer risk estimates. Not only does this research fail to include dose from residual radiation, fallout and food web sources, but it also fails to include medical X-ray data for each survivor. Radiation "dose" in these studies excludes all ionizing radiation exposures except that from the original flash of the bomb. Many survivors were part of special investigations requiring medical X-rays, the Japanese medical doctors X-ray the survivors at their yearly medical examination, the American researchers X-ray them every second year.

Although the A-bomb scientists have now admitted that more cancers were caused per unit dose of radiation than previously thought, ICRP has now given itself risk reduction factors for slow dose rate and low dose. This introduction of an unsubstantiated "correction factor" gives evidence of the inadequacy of the data base to answer important questions about worker and public exposures, which are almost all at low doses and slow dose rate. It also indicates that the ICRP knows that it is inadequate. There is no supporting human evidence for this reduction of the risk factors, and considerable evidence that it is not warranted. (Ref. 9).

I do not have time to go into all of the myriad details involved in forming my judgement, since I have worked in this field for thirty years, but I would generally recommend the article: "Inconsistencies

and Open Questions Regarding Low-Dose Health Effects of Ionizing Radiation", by Rudi H. Nussbaum and Wolfgang Kohnlein, and also the fine research papers published by Dr. Alice Stewart on this subject, and on ABCC failure to correct their data for the Healthy Survivor effect. It is my professional opinion that the slow dose rate - low dose reduction factors used by ICRP (and UNSCEAR) are not justified. It is also my professional opinion that the fatal cancer dose rate for an exposure of one hundred Person Gray should be conservatively set at 20, rather than the current 5 as recommended by ICRP. The direct extrapolation for Atomic Bomb data to low dose exposure would predict 17 fatal cancers per Person Gray exposure. They obtain this estimate in spite of losses through failure of death certificate information and elimination of all deaths prior to 1950. This, in the face of under reporting, is in close agreement with nuclear worker data, and should not be reduced with this Dose-Dose Rate Reduction Factor.

### **BIOLOGICAL MECHANISMS AND ENDPOINTS:**

In the early 1950's, when it was generally recognized that using the erythema dose, the dose which actually burnt the skin, was not adequate as a guide to radiation protection, many different biological endpoints were proposed as guides to regulatory standards: reproductive problems, tumors, congenital malformations, cataracts, blood disorders. Other possible biological endpoints were added later: obesity, hormonal disruptions, auto-immune diseases, developmental disorders, mental and physical retardation. ICRP decided that people should only be concerned about fatal cancers, and the only biological mechanism to be considered would be direct damage to DNA. Most of the other endpoints are dismissed as transient, not consequential, not damaging of the gene pool, or not fatal. This is an administrative, not a scientific decision, with which we may well wish to disagree. Even with respect to fatal cancers, those which were promoted or accelerated by the radiation exposure are not counted, because they are not considered to be "radiation induced" (Ref. 10).

Hiroshima and Nagasaki studies of non-cancer effects of exposure to ionizing radiation are either very poor or non-existent. I remember my frustration when I first looked for data on the relationship between exposure to radiation and adult onset diabetes. Diabetes among Hiroshima males had shown a linear trend with dose for causing death (Ref. 11). Since diabetes is not normally a first cause of death, one could well question the relationship of radiation with incidence rate of diabetes. When I located the research paper from the ABCC, I was astonished to find a bold statement that diabetes shows no relationship with radiation exposure in the early part of the paper. There is no supporting evidence for this statement. The remainder of the paper is devoted to a discussion of diabetes among A-bomb survivors with no further mention of or reporting of their doses. Reference is made to negative findings of atomic bomb research in order to discourage further research into the relationship between diabetes and radiation. Diabetes rates are extremely high in the nuclear fall out areas of the Pacific, downwind of the Nevada Test Site, and in areas of heavy fallout in the Arctic. However, no research has been done into the possible causal links with nuclear fallout.

The US studies of the health affects of nuclear fallout were carried out in the Marshall Islands, not (as noted earlier) in Japan (Ref. 12). They are much less publicized. The US began testing nuclear bombs at Bikini Atoll in the summer of 1946, before the territory had been given to it by the UN as

a "Strategic Trust Territory". The world community knew that it was the intention of the US to use this territory for nuclear testing, but chose to look the other way. The Australian Ambassador was the exception, and he chose to resign from the UN over this issue. Other nations could hardly have failed to notice! Australia merely replaced their Ambassador, the US was given its testing site in 1947, and everyone looked the other way as the US and UK conducted nuclear tests in the Pacific and Australia (Ref. 13).

On March 1, 1954, the US exploded a 15 Megaton hydrogen bomb at Bikini, and no one informed the Rongelap People, who lived downwind of the testing site. The Weather men stationed at Rongerik Atoll, slightly further away from Bikini than Rongelap, have publicly testified that they warned the military that the winds were traveling in the direction of inhabited Atolls. The US Navy ship, Gypsy, stationed just off the tip of Rongelap, was ordered to move away from the fallout area, but the Rongelap People were not warned.

About 72 hours after the heavy fallout on Rongelap, which polluted the land, drinking water and food, the Rongelap People were evacuated to the Kwajalein Atoll military base for medical examination and care. Many suffered severe radiation sickness, burns, epilation (hair loss), and depleted blood counts. They were forced to stay on Kwajalein for three years, until the US Military declared their Atoll again "safe for habitation". In moving this population of about 87 people back to the Rongelap Atoll, the US chose a population of relatives (Rongelapese who were not on the Atoll at the time of the fallout), matched for age and sex, to return to the Atoll as a "control" group for their research.

Money appropriated by the US Congress for the health of the Rongelap People was given to the Brookhaven National Laboratory for their research program. The Laboratory purchased and outfitted a ship which they used in the summer to travel from Long Island, New York, via the Panama Canal, to the Marshall Island, which is about half way between Hawaii and Japan. Their medical program consisted primarily in conducting blood tests of the Rongelap "cases" and "controls", and examinations for thyroid nodules or other thyroid abnormalities. The medical "care" given to the Marshallese consisted of referral slips to local health professionals noting some medical problem which had been found during the examination and recommending medical diagnosis or treatment (often not available in the substandard facilities in the Trust Territory). If they found a thyroid abnormality, this Brookhaven team would recommend flying the Marshallese to the Cleveland Clinic in the US for a thyroidectomy, calling this preventive surgery (preventing thyroid cancer by removal of the thyroid gland).

In 1978, the US Department of Energy conducted an extensive investigation of the residual radiation on Rongelap Atoll. The Rongelap People after seeing the reports of their still contaminated Atoll and food web, evacuated themselves and began a struggle with the US Congress for cleanup and compensation. Finally in the late 1980's, the Congress agreed that the Island was still uninhabitable, although the experimental population had been living there from 1957 to May 1983, some 26 years. The nuclear scientists working for the US Department of Energy and the US Department of Defense claimed that the Rongelap People were irrationally fearful of the radiation and that their evacuation

was uncalled for. Eventually the Congress not only commended the Rongelap People, but they ordered a cleanup of the Atoll to a level guaranteeing that exposures of the people would not exceed 0.25 mSv per year, well below the 5 mSv per year standard used in the US. This same standard for cleanup was used by the US on the Johnston Atoll, another US nuclear test site in the Pacific.

The medical examination of the Rongelap People included many reports of "monster" and molar births. According to the People they actually began to photograph these abnormalities, which at first they had hidden thinking it was their own fault to have such abnormal pregnancies. When the photographs were shown the American researchers, the pictures were seized. They burned them in front of the people saying: "This is what we think of your evidence". We heard this story from many different people on the Atoll.

In a cross sectional study which we undertook in 1988 (Ref. 14), we included 297 children, 134 adult females and 113 adult males, randomly chosen from Rongalapese in the US DOE "exposed" category, i.e. in the actual fallout, "control" category, i.e. relocated on the contaminated Atoll with the exposed group in 1957, and "neither" of the above, and their children. We found the following proportions with serious chronic illness among adult Rongalapese born prior to the 1954 hydrogen bomb detonation:

Category of Exposure:	Males	Females
Exposed	88.5%	88.6%
Controls	63.6%	76.8%
Neither	55.6%	58.1%

Serious congenital disease or malformation in living children (realizing that with the substandard medical facilities many were miscarried, stillbirths or infant deaths):

Category of Parental Exposure for children 15 years or under in 1988 (born since 1973):

Exposed*	15.3% with serious congenital diseases or malformations
Controls	21.0% with serious congenital diseases or malformations
Neither	8.3% with serious congenital diseases or malformations

\* This category had a higher rate of miscarriages and still births. There were 59 (1.6 grandchild per adult) offspring in this category, while the other two categories included 81 (4.1 grandchild per adult) and 84 (3.1 grandchild per adult) children respectively.

Category of Parental Exposure for those 16 to 34 years old in 1988 (born between 1954 and 1972)

Exposed**	No children
Controls	2.1% with serious congenital diseases or malformations
Neither	2.0% with serious congenital diseases or malformations

**\*\*** There were only 13 live children (0.36 per adult) in this survivor group, whereas there were about 50 (48, 2.4 per adult and 51, 1.9 per adult) respectively representing the other two exposure categories.

In the survivor population, those over 35 years of age in 1988, 2.4% were found to have congenital diseases or malformations. Using the three age groups as roughly representing three generations of Rongelapese, those exposed, their offspring and the third generation, we find some startling changes in health parameters:

#### **THYROID RELATED PROBLEMS:**

Category:	Exposed	Controls	Neither
Alive in 1954	58.3%	5.0%	18.5%
First Generation Offspring	----	8.3%	11.8%
Second Generation Offspring	1.7%	----	1.2%

It seems that we should have expected the thyroid abnormalities at Chernobyl! However, the world medical community was completely unprepared for the crisis since this Rongelap data was not widely known by the non-US Government scientists.

#### **TUMOURS AND CYSTS:**

Category:	Exposed	Controls	Neither
Alive in 1954	25.0%	5.0%	7.4%
First Generation Offspring	15.4%	4.2%	7.8%
Second Generation Offspring	----	2.5%	1.2%

#### **HEART PROBLEMS:**

Category:	Exposed	Controls	Neither
Alive in 1954	22.2%	15.0%	7.4%
First Generation Offspring	7.7%	6.3%	3.9%
Second Generation Offspring	5.1%	13.6%	3.6%

#### **MENTAL AND NEUROLOGICAL ABNORMALITIES:**

Category:	Exposed	Controls	Neither
Alive in 1954	2.8%	----	3.7%
First Generation Offspring	7.7%	6.3%	2.0%

Second Generation Offspring	1.7%	----	1.2%
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These figures likely indicate the teratogenic effects on first generation born on the contaminated Atoll after the relocation there of the exposed and control population in 1957.

#### REPRODUCTIVE PROBLEMS EXPERIENCED BY WOMEN:

Category:	Exposed	Controls	Neither
Alive in 1954	66.7%	60.0%	46.2%
First Generation Offspring	25.0%	36.4%	22.7%

#### ADULT ONSET DIABETES:

Category:	Exposed	Controls	Neither
Over 35 years of age	11.5%	7.9%	5.2%

It seems clear that limiting ones concern to fatal cancers may provide neat mathematical simplicity, but it is unrelated to the reality of the suffering of the survivors of radiation exposure.

The Investigation Committee of Atomic Bomb Victims of the Hannan Chuo Hospital, Osaka, Japan, undertook a study of 1,233 atomic bomb survivors (554 males, 678 females, and 1 unknown) living in Osaka (Ref.15). This study was undertaken in 1994, and the average age of the survivors was 59.5 years. The survivors were compared with the data for the same age group of the Standard Japanese Population (Ref 16).

More than 90% of the survivors were under medical service and more than 50% experienced frequent hospitalizations, about 2.5 time higher than in their unexposed peer group. They found the following:

DISEASE	% SURVIVORS WITH DISEASE	RELATIVE MORBIDITY TO THAT OF GENERAL PUBLIC
Lumbago	28.4%	3.6
Hypertension	23.9%	1.7
Ocular Disease	18.0%	5.0
Neuralgia and Myalgia	12.3%	4.7
Leukopenia	12.1%	13.4
Gastritis	9.9%	4.5
Gastroduodenal Ulcer	9.8%	4.7
Ischemic Heart Disease	9.8%	4.7
Liver Disease	9.0%	6.4
Diabetes	8.2%	2.7

Similar findings have been reported at international NGO forums on the damage and its aftermath for atomic bomb survivors in Japan, and documented in the 1986 report of the Association of Victims of Atomic Bombs of Japan. Recently the RERF has acknowledged that in their limited survivor group they have found excess relative risk of cerebro-vascular and cardiac diseases, and gastro-intestinal diseases, especially liver disease, in those who were younger than 40 years at the time of bombing (Ref. 17, 18 and 19). One can only conclude that the official radiation studies were either incompetent to report these diseases or uninterested in them.

In the early 1970's, when I was part of the analytical team working on the Tri-State Leukemia Survey, I noticed the remarkable statistical regularity of the increase of non-lymphatic leukemia incidence in the population with increasing age. From age 15, when the incidence rate is at a minimum and childhood cancers have played out, one finds an increased rate of about 5% per year of these leukemias. I found the same compound interest type increase in non-lymphatic leukemias in the general population with increased usage of diagnostic medical X-rays, about 4% for trunk examinations. Therefore, I posed a new research question: What exposure to medical X-rays is comparable to one year of natural aging for increasing the risk of non-lymphatic leukemia? I found that the answer was dependent on the part of the body exposed to the X-ray, which turned out to be the amount of the bone marrow exposed by the particular X-ray procedure (Ref. 20).

With one more important piece of information, namely that medical X-ray is measured by the mR in air at skin entrance (rather than by tissue or bone marrow dose as used by the physicist), I will telescope some ten years of research into a few short conclusions:

- For X-ray of arms or legs, and dental X-ray, it requires an accumulated dose of 4000 mR to increase the risk of non-lymphatic leukemia the same amount as one year of natural aging.
- For chest X-ray, it requires an accumulated dose of 1670 mR at skin entrance to simulate one year's natural aging for increasing non-lymphatic leukemia rate.
- For abdominal X-ray it requires an accumulated dose of 1000 mR in air at skin entrance to simulate one year's natural aging for increasing the non-lymphatic leukemia rate.
- The corresponding bone marrow doses for these three sites and these mR doses are: 0.64, 0.72 and 0.83 mSv.
- This is clearly consistent with measurements of the external annual effective dose equivalent for natural background radiation: 0.65 mSv in UNSCEAR 1982 (for normal parts of the world); 0.81 mSv in Solon et al. 1958 (for 124 US cities); 0.61 mSv in Beck et al. 1966 (for 210 locations in the US).

I called this generalized effect of X-ray on the ability to resist non-lymphatic leukemia an "acceleration of the aging process" (Ref. 21). This is a less sophisticated term than "genome

instability", but I think that I was measuring the same phenomenon in humans exposed to diagnostic X-rays.

Another important point of this research is that although medical X-ray is low dose, it is given at a fast dose rate, a matter of seconds, whereas the natural background dose is delivered at a very slow rate, spread over the course of a year. There is obviously not a dose rate difference, contrary to what the ICRP would have us believe.

In other research on the Tri-State Leukemia data, I used the natural aging equivalent of each persons medical X-ray exposure history, and added it to their chronological age to obtain what I called the person's "biological age". This was then used in the standard age adjusted statistical procedures rather than the chronological age. It served to elucidate many problems of apparent inconsistency in the data, and proved to be a valuable tool in understanding the complex relationships between environmental factor influencing leukemia rates in a large population. For this reason, namely, its general nature as a factor requiring control (just as one must control for age in epidemiological research) I believe that the aging effect, or genome instability, has broader consequences than just increasing the rate of non-lymphatic leukemia. Again, this implied a need to expand the biological endpoints and low dose mechanisms of concern when dealing with exposure to ionizing radiation.

In addition to these general affects on the whole organism, there are micro-biological effects and biomarkers of exposure which have been neglected by the ICRP because of their focus on cancer death and only one mechanism, namely, direct damage to the DNA molecule initiating a malignant growth. Professor Michael Vicker, University of Bremen, has documented the acute radiosensitivity of blood to micro-Gray doses of radiation, causing the arachidonic acid cascade (Ref. 22). Rather than trying to extrapolate the DNA damage hypothesis from the high dose exposures to radiation into theoretical happenings in the low dose range, researchers would do better to expand the mechanisms studied to include those which actually occur at the low dose and their sequelae.

With all of the sweeping changes which have occurred in biology and microbiology since the 1952 discovery of DNA by Watson and Crick, radiobiology has stayed focused on cancer and direct damage to DNA. Other branches of biology have expanded to consider the entire cell, systems influencing cellular behavior including functional levels and coupled feedback reactions of networks of inter- and intra- cellular responses regulating cell communication. Without a holistic view of biology and physiology, radiobiology has been consumed with detail and elaborate mathematical picture of the small world which was delimited by the very first administrative decisions of the nuclear bomb era.

In an organism, cells communicate with one another through the exchange of specific information, for example through a hormone, and the translation of this signal into intracellular messages. Paracrine (hormones secreted from tissues other than endocrine glands) and endocrine hormones are unable to pass through cell membranes. Therefore their information (the hormone) requires a cellular receptor on the outside surface of the cell, a transmembrane signaling that is connected to the receptor, called a "second messenger-generating enzyme", and a correct interpretation of the second

messenger system. Various second messengers are released into the cell after stimulation of a particular receptor enzyme system, and which systems may be activated depends on the genetically determined receptors possessed by the cell. This communication system between cells in complex systems, can be modified, for example by phosphorylating particular proteins, and two second messengers can interact through feedback and cross talk. Ionizing radiation causes many interferences and disruption in this delicately balanced intercellular communication system. In radiobiology, these problems are dismissed and assume to be either trivial or perfectly repaired. Ionizing radiation induces oxidative stress, something admitted by radiobiology but discussed only in terms of its thermal effects. This same oxidative stress induces measurable inflammation, including a massive cascade of fatty acids in various states of oxidation. These mediate inflammatory reactions in the blood and other tissues, such as blood vessel endothelium, and function as second messengers, even controlling such things as pain and chemiluminescence.

The perturbation of cellular communication, regulation and homeostasis by low doses has major consequences for human health and development. It is irrational, as the physicists are now doing, to count on the failure to observe high dose effects at low doses as "proof" that such doses are "safe". DNA damage is a statistical phenomena, called stochastic by the physicists, while the inflammatory response is non-stochastic, or deterministic as it is now called. Unlike skin burns, these internal inflammatory responses occur at microGray doses. The ICRP assumes that deterministic effects do not occur below 500 mGy doses.

The ionizing radiation stimulations are "illicit" in the sense that there is no equivalent stimulation of the arachidonic pathway after non-radiological physiological stimulation, making it pathogenic in character, difficult for the body to regulate and return to homeostasis. This response activates the monocytes, which kill themselves by the oxidants they produce, often ending up as pus along with their digested cellular victims. They can endanger the host by killing other tissue, for example, transplants or infarcted heart tissue.

Activated monocytes are carcinogenic, provoking hitherto latent oncogenic systems and genomic errors to replicate. This may well be one of the mechanisms by which cancers were increased within the first ten years after the Chernobyl disaster. These cancers were dismissed by the IAEA as not radiation related because the ICRP required latency period of ten years had not been completed. These were radiation promoted or accelerated cancers, not radiation induced cancers. Again, we see ICRP recognizing only radiation induced cancers, whereas the victim will experience both mechanisms as due to the disaster.

### **HORMESIS:**

Recently, in a concerted effort to raise the permissible levels of radiation for workers and the public, members of the Heath Physics Society have been actively promoting their theory of Hormesis, namely, that low dose exposures to radiation induce "beneficial" effects such as longevity, robustness, radio-resistance and increased growth. The use of the term "beneficial" implies a judgement, not a scientific fact. Experiments backing these hypotheses have been difficult to reproduce and

definitions of "beneficial" have been controversial and appear very subjective. Claims of low dose hormesis have frequently been based on high dose observations, and the only mechanisms offered for these effects has been speculation on repair overshoot at the cellular and genome level. Cell growth as "hormetic" is the most troubling claim, since illicit growth stimulation signifies catastrophe for biological organisms.

What has been sorely neglected in this public relations battle, is that low dose radiation at the cellular level must necessarily affect a large range of molecules in the cellular communication system in any particular cell type. In order to produce one "good" effect, one must endure many other unwanted "bad" effects which will in the long run claim a physiological price perhaps significant, although they evolve to a clinically observable level more slowly (Ref. 23).

Many of the phenomena which have been attributed to radiation exposure by the victims, and those scientists and physicians who have studied the problem from the victims point of view or simply from the available information, can be explained by the low dose effects on inter- and intra cellular communication. In particular, this includes: the high rate of cardiovascular disease deaths in radiologists (Ref 24); the deaths of infants in the higher fallout areas after the Chernobyl disaster in Germany (Ref. 25); the increased rate of low birth weight infant deaths which I documented in Wisconsin, statistically associated with increases in off gas releases from neighboring nuclear reactors (Ref. 26); and the higher than expected cancer mortality rates for nuclear workers (Ref. 27 and 28).

In therapeutic irradiation to kill cancer cells, there are often unwanted reaction in non-irradiated tissues. Sometime this secondary effect is lethal. Under the dominant theory that the only damage of concern is DNA damage, there is no remedy after the exposure. However, experience in hospitals has shown that corticosteroids, which inhibit one of the second messenger reactions, and aspirin like compounds, which inhibit the inflammatory response, can reduce these secondary effects. They have demonstrated that these conditions are treatable.

The internal "sunburn" attributable to low dose ionizing radiation exposure may perturb homeostasis, and aggravate pathological conditions such as allergic or arthritic diseases, heart and circulatory disfunction, and cause death for the embryo, fetus or infant critically dependent on timed signal exchanges between cells for proper development.

It may also be true that in subsistence communities, such as was reported for India, children are more sensitive to the low dose effects. The children in five Indian villages downwind from two nuclear reactors demonstrated four-fold higher rates, statistically significant levels, of congenital malformations than a comparable subsistence control group 50-60 kilometres away. Adults (born before the operation of the nuclear reactors) showed comparable levels of congenital malformations (Ref. 29). There have also been documented reports of teratogenic effects after the Chernobyl disaster (Ref. 30). This has very serious implications for the current push to market this unwanted technology in the economically developing countries.

My own research has pointed out the dramatic reductions of monocytes in ionizing radiation exposed populations in many parts of the world (Ref. 31). It seems to be clearly a biomarker for exposure, similar to the way a sun burn is a biomarker for exposure to visible and ultra violet light. I believe that what I am measuring is both a response to low dose radiation as described by Vickers, and also an effect due to the radiosensitivity of the stem cells in the bone marrow which produce the monocytes. These stem cells, subjected to chronic irradiation by the radionuclide incorporated into bone (strontium 90, plutonium, uranium, radium, lead 210), become depleted, clinically resulting in iron deficient anemia and depression of the cellular immune system.

I hope that I have shown that the very narrow focus of ICRP on one biological mechanism of damage to one type of molecule, namely DNA, and neglect of all other mechanisms and molecular damage from ionizing radiation, is scientifically abhorrent and practically very prejudicial to the victims of radiation. There are now attempts to further restrict this narrow focus to health effects due to doses above 100 mSv, through claims of "hormesis" below this dose. The victims must try to fit their problems into the narrow categories "accepted" by the ICRP. It should be the other way around, namely the ICRP is expected to recognize and protect against all mechanisms, damage to all important molecules, and the serious consequences of such damage for human health subsequent to all doses of radiation.

It should also be noted that studies done in Russia after the Chernobyl disaster, point to doses which are below the stimulation of the cellular repair system. That is, at very low doses of radiation the cellular repair mechanisms are not stimulated and the damage goes unrepaired. This would imply "J" shaped curve for effects at low doses (Ref. 32).

#### **ADEQUACY OF RESEARCH INTO NON-CANCER EFFECTS:**

Unfortunately, because of the professional isolation of radio-biologists from their colleagues in microbiology, biology and physiology, they have spent their time in elaborate mathematical modeling of the basic narrow focus determined in 1952: namely reconciling the different types of radiation and energies of the transformation events, relating partial body exposure to whole body exposure, setting tissue weights to reflect the fatal nature of the induced cancers. They have missed the examination of subtle low dose exposure mechanisms, investigations into the reasons for differences in radiation sensitivity between different tissues, different people and the same person at different periods in their life.

The non-cancer effects of radiation have largely been studied outside of the generous funding mechanisms of the nuclear establishment, and these studies often cannot produce accurate dose estimates. For example, the whole field of teratogenic effects of radiation. These effects are well known, and have been demonstrated in medical X-ray case and even more clearly in Kerala, India, and Chernobyl, Ukraine. However, if you have made an administrative decision that there are only two categories of radiation effects worth considering: direct damage to the Standard Man, and damage to the population gene pool, then this damage is of no concern and dose responses are not obtained. Teratogenic damage, embryonic and fetal losses, as well as still births, apparently do not

count, because they do not effect the population gene pool and are not an economic cost to society. These damaged offspring never pass on the defect to future generations.

I did a small study on the Tri-State Leukemia data to see if there was a deficit of births in the "irradiated in utero" sub-sample. I found that in the control children, those without leukemia or other life threatening disease, matched to the case children for age, sex and geographical location, there was a deficit of children in every irradiation category (Ref. 33). This is highly significant on a 1% level, that is, it would happen by chance in less than one of a hundred such studies. In all, assuming that the unirradiated children gave the population distribution of pathological factors, and the children with no pathological factors gave the distribution of irradiation categories, 259 children would have been expected in the control population, but there were only 223, a loss of 26 (10%) of the sample. The children with leukemia, on the other hand, were over represented in each of the radiation and pathology categories. There were 151 children, while only 130 were expected, an excess of 21 (14%). Both of these groups of children were controlled for Mother's earlier pregnancy loss and pathologic factors. One can assume that the excess was attributable to diagnostic X-ray at doses below 1 mSv. Usually prenatal X-ray examinations are assumed to give a dose of 0.5 mSv to the fetus. This is one half of the yearly dose to the public permitted by ICRP. Investigation into the mechanisms behind this reproductive loss has been minimal or non-existent.

Research into the genetic effects of exposure to ionizing radiation has also been unsatisfactory, even though this is on the ICRP administrative list of detriment concern. For example, as early as 1957, the World Health Organization identified the population exposed to high background radiation in Kerala, India, as the best population in the world for studying the genetic effects of radiation (Ref. 34). This was never followed up with action until a group of independent researchers with a small grant from the World Council of Churches undertook a study in 1988. This data has now been collected but needs more input of money for main frame computer analyses, and publication of the findings. We do know that on the high background monozite sands, with chronic exposures between 3 and 30 mSv per year, there is four times the rate of Down's Syndrome, twice the rate of other mental retardation, epilepsy, congenital blindness and deafness, deformities of the long bones and infertility, than is found in the matching control group on normal background (Ref. 35).

It is scientifically outrageous to keep stating that the RERF research found no genetic effects of radiation! Atomic bomb researchers were aware of the fact that their data base was inappropriate. Their research is clearly poorly designed because of their odd matching of cases and controls, their failure to correct for healthy survivor effect and the shortness of time since exposure, which can mask intergenerational effects. Yet the ICRP has failed to call for support for the research which is universally agreed upon as most likely to show the effects of chronic intergenerational exposures.

Meanwhile, the genetic problems has been reduced by ICRP administrative decision not to deal with recessive genetic damage, or diseases with genetic components, but rather to limit consideration of genetic damage to the most obvious autosomal dominant and X-linked defects, and chromosomal diseases. The risk estimates being used for genetic damage are derived from rat studies. Sometimes the genetic effects "of concern" are limited to the first generation offspring under the pretext the

damage to subsequent generations does not cause sorrow to the individual exposed during their life time!

Current urgent research needs in the area of radiation health and safety includes:

- Funding of serious analysis of the Kerala data, with full involvement and credit given to those who have carefully collected this data without proper financial support from either governments or the nuclear industry.
- Research into the dose response estimates appropriate for teratogenic effects of radiation and inclusion of these effects in the administrative category of "detriments".
- Research into dose response relationships between radiation exposure and the occurrence of: cysts; blood abnormalities; autoimmune diseases; hormonal disruptions; reduced fertility; skin cancer (including non-melanoma), and the so-called "transient" effects of exposure which disrupt homeostasis.

One would expect that such research, seriously undertaken, would lead to the use of genetic and teratogenic damage as the basis of radiation protection standards.

In the current application of radiation protection standards, for example at nuclear reactors, it is important to change the focus from maximally exposed individuals (usually the Standard Man who works out of doors near the facility) to maximally susceptible individuals (the embryo, fetus and baby being fed with contaminated milk), in order to truly protect against the most severe detriments. Standards should be protecting the public against the harmful effects of radiation exposure both to the individual (including those unborn) and to the gene pool.

The elegance of the mathematical theory should not take precedence over common sense protection of the most vulnerable.

#### **NEED FOR RADIATION PROTECTION STANDARDS:**

I would not like my remarks to be construed to mean that regulation of radiation exposure should not take place. It is of course necessary that standards be set. I believe that the standard setting should be recommended by a professionally established open body, with credentials in occupational and public health. The ICRP is profoundly undemocratic and unprofessionally constituted. It is self-appointed and self-perpetuated. Certainly a recommending body could be composed of individuals elected from professional societies such as international associations of professionals trained in occupational health, epidemiology, public health, neonatology, pediatrics, oncology, etc. Some members could be recommended by the WHO and the ILO.

An organization of users of radiation, such as ICRP, being asked to set standards is like inviting the tobacco industry to regulate tobacco! ICRP is organized by its By-Laws to include only users and

national regulators (usually coming from the ranks of users) of radiation.

If it is decided that fatal cancer incidence rate should be the biological endpoint on which the regulations are based, and I do not accept this as the best indicators of problems, then the radiation industry needs to conform to the same standards of injury as is used for regulating the chemical industry.

The State of Minnesota, in the USA, decided that a nuclear waste dump should not be able to cause more than one cancer (fatal or non-fatal) over the life-time (70 years) of an exposed person. This is the standard which the State used for chemical polluters. Based on this, a criteria of no exposure of the public above 0.0005 mSv per year was derived by the State Department of Health. This Standard is being enforced in that State, although it is ten thousand times lower than the current permissible dose to the public per year under US Federal Law, namely 5 mSv per year.

In Ontario, the Advisory Committee on Environmental Standards (ACES) expressed astonishment that the nuclear industry was permitting itself to pollute the drinking water with up to 40,000 Bq of tritium per Litre, under the 5 mSv per year federal radiation dose limit for members of the public. When the ICRP reduced the recommendation to 1 mSv per year, the industry agreed to lower the permissible level of tritium in water to 7,000 Bq per Litre. When the ACES used the industry risk estimates for calculating the expected number of fatal cancers considered to be "permissible" under this Standard, they called for an immediate reduction in permissible levels to 100 Bq per Litre, with a further reduction to 20 Bq per Litre within five years. This was based on the standard setting used for toxic chemicals. This means the radiation protection guide line allows 350 times more fatal cancers than chemical standards would allow.

While I understand mathematically why the nuclear industry, dealing with a mixture of radionuclides sets such unreasonably high permissible values, I see also that these high values are used for public relations reasons to assure the trusting public when there is a spill or abnormal incident at a reactor. Stating that the exposure was less than 10% of the permissible dose, sounds reassuring! Yet if one knew that the permissible dose was 350 times too high based on cancer deaths caused, 10% would be seen as 35 times too high. It is in the interest of the nuclear industry, hiding behind ICRP, to carry on the subterfuge that "permissible" implies "no harm".

The ICRP assume no responsibility for the consequences attributable to a country following its recommendations. They stress that the Regulations are made and adopted by each National Regulatory Agency, and it merely recommends. However, on the National level, governments say they cannot afford to do the research to set radiation regulations, therefore they accept the ICRP recommendations. In the real world, this make no one responsible for the deaths and disabilities caused!

In ordinary public health practice, an industry can be called "safe", if it causes the death of less than one person per million exposed to it per year. Using the nuclear industry's own estimate of risk of fatal cancer, and the 1990 ICRP recommendation to keep exposures of the general public below 1

mSv per year, there is an expectation of 50 cancer deaths per year per million exposed. I believe that the risk estimate used by ICRP is too low by a factor of four, based on research done at the low dose and slow dose rate exposure level. This means the number of deaths per year may be as high as 200. These 200 deaths are likely to be predominantly deaths of women and children, and many of the cancers will be expressed clinically after the local reactor is decommissioned. Women have more cancers per unit exposure than do men because of their high risk breast and uterine tissue, and also because they are more susceptible to radiogenic thyroid cancer than are males. Children pick up more radionuclides from the water and food web, incorporating more in bone because they are growing. Children have less mature immune systems, and have a longer life expectancy during which the cancers of longer latency period can develop. It is the men over 50 years who have the smallest risk!

It would certainly be worthwhile for the Parliament to appoint a serious study of radiation protection standards, considering the current death estimates together with the potential breadth of biological endpoints which are truly of concern to the general public. Mental retardation, epilepsy, blindness and deafness are tragedies as well as social expenses never assumed by this industry. Infertility is spawning expensive in vitro fertilization clinics throughout the world. The economic costs externalized by this industry are very large.

I would personally be opposed to leaving the regulation of radiation completely to each national government, with an international recommendation. The nuclear industry has been trying for several years to have the regulations relaxed even further, and I understand that the next released report from the United Nations Scientific Committee on the Effects of Atomic Radiation (UNSCEAR) will be devoted to the "belief" in hormesis, the "benefit" of exposure to low level radiation. As a plenary member of the Health Physics Society, I have watch this movement within the industry expand over the past few years. The rallying cry is: "Put your mouth were your money is". Health physicists are trying to keep this industry alive in any way they can. Making radiation more acceptable to the public is part of that plan. In the face of such organized opposition to regulation, it will be necessary to establish an honest, prestigious organization which speaks to health - both of humans and of the ecosystem. It should be independent of the vested interest of users of radiation who make their living from this use. It should not attempt risk-benefit trade-offs, but only clarify and quantify the risks.

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